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Editorial Notes

LAST June we spoke of the need for appointing immediately a Director of Antiquities for the Aden Protectorate, and we mentioned the fact that the Sudan Government had at last made a similar appointment for its own territory. For this good deed it deserves the thanks and congratulations of all archaeologists and anthropologists. But, good as it is, it does not go far enough. The Sudan covers a vast area. It is not, like Egypt, a narrow densely populated Nile-corridor, but a huge plain watered by annual summer rains and dotted with villages and granite ' gebels ', standing out like pink islands in a sea of mimosa thorn. Throughout the whole of this area, more particularly in the valleys of the Nile and its tributaries, are the remains of ancient settlements. As cultivation spreads these are progressively endangered. Every day probably something irreplaceable is destroyed and a fragment of history gone for ever.

In the past the only archaeological post in the Sudan has been that of Conservator of Antiquities. The post was not a full-time one, the work being done in his spare time by a government official ; the last three persons to hold it were the Director of Education, an Inspector of Education and the Government Geologist. With the creation of the new full-time post of Commissioner of Archaeology and Anthropology, that of Conservator of Antiquities has been allowed to lapse ; but it is intended to revive it as soon as the country can afford it ; and we may say that the creation of this post at the present difficult moment in its

history reflects the greatest credit upon the Sudan Government. Those who know anything of the Sudan, whether directly or through the admirable medium of *Sudan Notes and Records*, realize that the Government is sympathetic and that it can be relied upon to carry out its intentions at the earliest available opportunity.

At present the duties of the Conservator of Antiquities are being performed by the Commissioner, but he is sadly overworked. In addition to supervising and helping the anthropological investigations of government officials, missionaries and specialists in the provinces, the new Commissioner is finding his time more than fully occupied in looking after the various museums (particularly the archaeological and ethnological collections) and the innumerable ancient sites—ancient Egyptian, Ethiopian, Meroitic and Christian—dotted at intervals of a few miles along the Nile Valley between Khartoum and Wadi Halfa ; and there are many others in the valley of the Blue Nile and elsewhere throughout the vast Sudan.

The protection of these widely scattered sites is as urgent as it is difficult. The sites are looked on by the natives as convenient quarries for stone and manure (*marōg*) ; and often, where elaborate temples have been constructed of sandstone, the stone has become so impregnated with salt that exposure to the atmosphere by excavation has in many cases caused the rapid disintegration of the fabric.

There is a crying need for a permanent museum at Khartoum to house in a worthy fashion (so that they will be protected both from the climate and the ignorant, and also made available for study) not only the country's share of movable finds from past excavations carried out by the Harvard-Boston expeditions, Oxford Excavations in Nubia, and the Egypt Exploration Society and others, but also masonry of historical or artistic value which cannot be safely preserved on its original site.

Hitherto the country has had to be content with one room in Gordon College in which is displayed a small, very valuable, and as yet

unpublished, collection of the smaller Sudanese antiquities dating between 1800 B.C. and A.D. 300. There is no room to display the additions to the collection that have been made in the last seven years : and these and other materials for student collections have been kept in boxes in temporary stores until such time as the country could afford a museum. It has however now been realized that it is impossible to keep many of the antiquities safely in boxes for an indefinite period. The packing and the boxes themselves deteriorate from the action of white ants, dry rot, etc., with resultant damage to specimens when moved ; and the damp atmosphere of the rainy season plays havoc with salt-impregnated stone and faience. The construction of a permanent museum being at present beyond the finances of the Sudan, temporary accommodation is being provided, so that next year it should be possible to make the whole archaeological collection safe from further deterioration, and available for students.

More than this however is needed ; a fairy godmother is urgently required by the Sudan, to provide it with a permanent museum with sufficient room to display on modern lines its unrivalled ethnological and archaeological specimens, to preserve for posterity representative pieces of masonry from the Egyptian, Ethiopian, and Meroitic sites of the country, and to enable gaps to be filled in the present collections, particularly in the representation of the prehistoric Stone Age cultures and of the Dark Age of the Sudan (*c.* A.D. 500–1500).

Such a gift would benefit not only the inhabitants of the Sudan, who are beginning to take an interest in their country's past, but also students of early African culture generally. In the past, waves of culture have repeatedly flowed through the Sudan into the lands that lie to the south and west of it : as was pointed out in a recent number of ANTIQUITY, in parts of the Sudan today there still survive (but only for a short time longer, in the face of the destructive advance of western influences) ancient customs and techniques that have already been obliterated in Egypt and elsewhere.

A permanent museum at Khartoum, if erected now, would ensure the preservation for posterity of such evidence of the past as survives to the present day. In a very few years it will be too late. The provision of an adequate staff and endowment would enable it to become a

centre for the compilation, publication, and exchange of information about north-east Africa as a whole. The geographical position of Khartoum is ideal for this purpose.

We wish the Sudan Government every success in the forward move which it has made by appointing its first Commissioner for Archaeology and Anthropology. But progress must not stop there. More is required than one whole-time official for this vast area. An adequate museum and staff are the next essentials and they are long overdue. (A geological survey which will make its publications available to workers in neighbouring countries is also urgently required). Anyone desiring further information is invited to communicate with the Commissioner, Box 178 Khartoum, who will be glad to answer any enquiry. Here is a magnificent opportunity for a public-spirited millionaire or for one of the great endowed Corporations. We hope these words may meet the eyes of some such before it is too late.

Anglo-Saxon Ship-burial in East Suffolk

A most important archaeological discovery has been made in East Suffolk. A ship-burial of an early Anglo-Saxon leader has been found, dating from about A.D. 600. The funeral offerings were put in a great rowing-boat, which had been drawn up from the water and placed bodily in the trench-grave dug for its reception. The boat had a length of 82 feet and a beam of 16 feet. Nothing remains of it but a pattern of iron clench-nails in the ground; but grave-goods of great archaeological interest accompanied the burial. These are now in the care of the British Museum authorities. It should be added that the body itself has not been found.

By the kindness of the responsible authorities we hope to publish a full account of this outstanding discovery as an illustrated article in our December number.

Domesday Water Mills

by MARGARET T. HODGEN

THE staple food crops of Saxon and Norman England included rye, barley, oats, millet, beans, and peas. Wheat also was not unknown. In fact, a few carbonized grains have been found upon Neolithic sites.[1] But, even as late as the thirteenth century, wheaten bread in more than one region of England was a luxury food, enjoyed by the mass of the agricultural population only on boon days or religious festivals.[2] The porridges and cereal soups, or flat-baked cakes and small loaves, which accompanied the peasants' daily cheese, sausages or herring, were usually made from a coarse meal composed of rye, or rye mingled in some proportion with wheat, barley, oats or beans.

From time immemorial until well into the period of the Saxon settlement corn was ground with a saddle-stone or quern, which formed part of the hearthside gear of every farm-stead and was operated by the women of the family as one of their household tasks. At some time during the Saxon period, however, probably in the eighth century,[3] a corn mill with mill stones turned by water power was introduced. Authorities are unable to agree upon the place of its first appearance in England[4] or the place of its invention.[5] Since none survives from this early date, they are also uncertain concerning many details of its structural design. But judging by water mills of a simple type which have been described by archaeologists as existing within recent years

[1] John Percival, *Wheat in Great Britain* (1934), 9–16; M. E. Seebohm, *Evolution of the English Farm* (1912), *passim*.

[2] Sir William Ashley, *The bread of our forefathers: an inquiry in economic history* (1928), 126 and *passim*.

[3] Richard Bennett and John Elton, *History of Cornmilling* (1899), II, 96–7.

[4] The earliest reliable allusion to the existence of a corn mill in England occurs for the year 762 in a charter granted by Ethelbert of Kent to the owners of a monastic mill situated east of Dover (Bennett and Elton, *op. cit.*, II, 96).

[5] L. Lindet, 'Les origines du moulin à grains,' *Revue archéologique* (1899), t. II et (1900), t. I; Marc Bloch, 'Avenement et conquetes du moulin a eau', *Annales d'histoire économique et sociale*, septième année (1935), 538–63.

in remote parts of Ireland, the Hebrides, the Shetland Islands and the Scandinavian peninsula,[6] the Saxon mill, like these, was what is known as the Norse or Greek mill. The water-wheel lay horizontally in or upon the water, turning on a shaft fixed to a stone in the bed of the river. The upper end of the shaft passed through the lower of the two quern-like grinding stones, and the mill-wheel, the shaft and the upper stone turned together. Apparently these ancient mills were extremely small and adapted for use only on minor streams or rivulets.[7] Mill stones were often no more than 18 to 36 inches in diameter and ground very slowly, one revolution of the water wheel producing but one revolution of the upper stone. Nevertheless, in 1086 there were over 5000 such mills in more than 3000 communities. A significant change had taken place in the material culture of England which removed the preparation of grain from the hearthside, altered the function of the housewife, introduced the miller and made the corn mill not only an important element in rural economy but a source of profit to those who erected them upon their holdings.

Where were these little mills located ? Upon what streams did Britons begin their long history of the use of water power ? In what regions or among what rural groups in Anglo-Saxon England did a far-reaching cultural and technological change of this character find welcome ? Conversely, what areas were peopled by a folk less hospitable to the new device ? The answers to such questions as these cannot fail to be of interest to archaeologists, historians, geographers, and economists. For the archaeologist and historian, they may throw some light upon debated problems of Saxon settlement and cultural change. For the economist and geographer, the distribution of Saxon water mills at the time of the Norman conquest forms a background against which to consider the utilization of water power in the later industrialization of England.

Domesday Book, referred to by Freeman as the first statistical document of modern Europe, is the obvious source to which those

[6] Bennett and Elton, *op. cit.*, II, 6–30 ; Joseph P. O'Reilly, ' Some further notes on ancient horizontal watermills, native and foreign ', *Proceedings of the Royal Irish Academy*, XXIX, Section C (1902–04), 55–84 ; E. Cecil Curwen, ' The Hebrides : a cultural backwater ', *Antiquity*, vol. XII, no. 47 (1938), 284.

[7] The mills in the Shetland Islands are described as ' being designed for a race of pygmies ', while those existing in Scotland in 1814, of which there were more than 500, ' were enclosed in a hovel about the size of a pigstye, . . . and incapable of grinding more than a sack at a time '. (O'Reilly, *op. cit.*, 62–4.)

interested in recovering some conception of the state of rural England at the end of the Anglo-Saxon period inevitably turn. This great inquest of 1086 was made by the commissioners of William I and ' had for its object to ascertain by local inquiry the amount at which each estate in the country was assessed to the land-tax called the Danegeld and . . . to obtain trustworthy data for a possible revision '.[8] Domesday Book is therefore in modern terminology a rate-book or valuation list. Its compilation was conducted with all the tax-collector's scrupulous attention to fiscal detail ; and ' so narrowly ', laments a contemporary chronicler, ' did the Conqueror cause the Survey to be made that not a single hide, yard-land, ox, cow, or pig escaped registration '.

Owing to the right of multure by which peasants were theoretically compelled to bring their corn to be ground at the lord's mill, the Saxon water mills were regarded by the Conqueror as an important source of revenue, and his commissioners were instructed to adopt a common formula for investigation which involved a statement of their location, ownership and value. In making a return for the manor of East Hanney in Berkshire, for example, the following typical entry is recorded : ' The same count holds Hanlei (East Hanney). Two freemen held it T.R.E. It was assessed at six hides ; now (it is assessed) at two hides. There is land for 5 ploughs. On the demesne there is 1 plough ; and (there are) 20 cotters with 1 plough. One serf is there, and 2 mills worth (de) 27 shillings and 6 pence, and 70 acres of meadow . . . '[9] In some cases where the mill was divided among two or more holders, the mill entry was often expressed in fractions, thus : Methelwolde (Methwold, Norfolk) ' was held by Stigand T.R.E. (as) 20 ploughlands . . . Then as now 2½ mills, 7 fisheries on the demesne '.[10] Great care was taken by the commissioners to distinguish between mill rents paid in cash and rents paid in eels or farm products.[11] And some mills were mentioned as winter mills, an indication that in summer the wheels were compelled by the lack of water to remain idle.

Despite the care with which entries were made, however, and the wealth of cultural data available, difficult critical problems arise in any attempt to interpret Domesday statistics and arrive at strictly accurate

[8] James Tait, *Domesday survey of Cheshire* (1916), 2–3.

[9] *Victoria History of Berkshire*, ed. by P. H. Ditchfield and William Page (1906), I, 345.

[10] *Victoria History of the County of Norfolk*, ed. by William Page (1906), II, 63.

[11] Rent was occasionally paid in iron, a fact which suggests that water power may have been used for purposes other than corn grinding.

results. In so far as the water mills are concerned, the chief impediment to accuracy lies in the incompleteness, in the present state of Domesday scholarship, of the identification of eleventh-century place-names with their modern equivalents.[12] Although Domesday expository literature is large, the document has only in recent decades received the attention of highly trained philological investigation.[13] Of the 34 counties covered by the Survey, only 28 have been recently edited and translated in such a way as to reduce errors in place-name conversion to a minimum.[14] In order, therefore, to complete an enumeration and geographical distribution of the mills existing in 1086 for this study, an appeal has been made to earlier and other materials. In the case of Dorsetshire, Eyton's pioneer study of the survey for that county, made in the seventies of the last century, was used.[15] For Wiltshire, W. H. Jones' *Domesday for Wiltshire*,[16] published in 1865, was employed, while for Cambridgeshire, Gloucestershire, Oxfordshire and Staffordshire, the lists of Domesday mills offered in Bennett and Elton's *History of Cornmilling* were utilized in conjunction with county place-name studies indicating present-day spellings.[17] As the result of this indirect procedure, the number of unidentified mill locations is somewhat high for several of the latter counties (Table 1), ranging from 6 per cent in Wiltshire to 12 per cent in Staffordshire. For England as a whole, however, the proportion of mills which cannot be located is only 2 per cent, or almost negligible.

The reader should be warned also that although an effort has been made to be exact, the occasional existence of the joint ownership of mills, expressed by the Domesday commissioners in fractions, may have led to error and could easily lead to difference of opinion. In the

[12] Sir Henry Ellis, *General introduction to Domesday Book* (1833), I, 41, n. 1.

[13] Due largely to the admirable work of the English Place-Name Society.

[14] These translations appear in the *Victoria Histories of the Counties of England* for Bedford, Berks., Buckingham, Cornwall, Derby, Devon, Essex, Hampshire, Hertford, Hereford, Huntingdon, Kent, Leicester, Norfolk, Northampton, Nottingham, Rutland, Shropshire, Somerset, Suffolk, Surrey, Sussex, Warwick, Worcester, Yorkshire.

A recent translation for Cheshire is in *Domesday survey of Cheshire* by James Tait (1916); for Lincolnshire in *The Lincolnshire Domesday and Lindsey survey*, translated and edited by C. W. Foster and Thomas Longley. Lincoln Record Society, 1924, vol. XIX.

[15] R. W. M. Eyton, *Key to Domesday* (1878).

[16] W. H. Jones, *Domesday for Wiltshire* (1865).

[17] Bennett and Elton, *op. cit.*, 147–162; W. W. Skeat, *Place-names of Cambridgeshire* (1901); H. Alexander, *Place-names of Oxfordshire* (1912); W. St. C. Baddeley, *Place-names of Gloucestershire* (1913); W. H. Duignan, *Staffordshire place-names* (1902).

present study, wherever the sum of fractional mills on a site is a unit and clearly indicates the presence of a single mill, it has been so considered and so computed. On the other hand, where the sum is fractionally more or less than one mill, the assumption has been made that although the commissioners may have omitted to mention all mill holders, a fractional mention nevertheless indicates the existence of a mill and has been so counted.[18] In other words, if a fraction of a mill was entered as subject to taxation, it has been assumed that a whole mill existed.

Geographical Distribution

Many students of the earlier periods of English history emphasize the influence of water supply upon human settlement. Man in the Bronze Age, according to Sir Cyril Fox, ' tended to live at the spring-line below the Downs, driving his herds and flocks on to the plateaux and hill-tops for pasture ; while in the Age that followed he moved his actual dwelling sites on to the uplands, using them both for arable and for pasture and driving his flocks down to the spring-line for watering '.[19] Other authorities assert that the river systems form a key by means of which the whole distribution of Anglo-Saxon settlements can be solved.[20] The Saxon invaders avoided Roman roads and penetrated the heart of England along the valley corridors, settling in villages higher and higher up the branching stream-lines.

[18] The existence of fractional or jointly owned mills is discussed by Maitland in *Domesday Book and beyond* (144) as a possible evidence of communal ownership in eleventh-century England. (For a somewhat opposing view see Marcel Thévenin, ' Etudes sur la propriété au moyen âge ; la " propriété " et la " justice " des moulins et fours ', *Revue historique* (1886, xxxi, 241–58). On the other hand, fractional holdings in mills, like fractional holdings in fields and peasants, may be ascribed to division by inheritance. (G. G. Coulton, *Medieval village*, 1923, 40, 45.) Such mills are usually mentioned as being divided between two holders or in halves. Less frequently three or four holders are indicated by the mention of a third of a mill or a fourth of a mill. In Lincoln, Norfolk, Sussex, and Surrey however a few mills are divided among five, six, seven or even eight holders. Norfolk, Lincoln, Wiltshire, Suffolk, Kent and Hampshire are the counties in which the greater number of fractional mills occurred and, according to Maitland, the counties in which joint mill ownership was most frequent. In Norfolk 87 manors are mentioned as sites of fractional mills ; in Lincoln 38 ; Wiltshire 23 ; Suffolk 15 ; Kent 13 ; Hampshire 11 ; Somerset 9 ; Oxford 7 ; Berkshire, Essex and Cambridge 6 ; Northampton, Sussex, Surrey and Nottingham 5 ; and the remaining counties less than 5.

[19] Sir Cyril Fox, *The personality of Britain : its influence on inhabitant and invader in prehistoric and early historic times* (1933), 69.

[20] E. Thurlow Leeds, *Archaeology of the Anglo-Saxon settlements* (1913), 18.

If the distribution of the 5624 Domesday mills is to be considered in any sense a clue to the boundaries of Saxon occupation, it should be noted (FIG. 1) that while mills were common enough on the streams south of the Severn and Trent, they are mentioned very infrequently on the rivers of northern England and almost not at all in Cornwall.[21] Apparently the Saxon utilization of water power began very abruptly in the west at a line formed by the river Exe and Exmoor Forest,[22] and from there followed a pattern dictated by the eastern radiation of fan-like water-sheds from the central elevation formed by the Mendip Hills, Salisbury Plain, and the Whitehorse Hills (Berkshire and N. Wilts Downs). The greatest mill density lay around this central plateau, in Wiltshire, Gloucestershire and Hampshire, and at the eastern termini of the fan spokes facing the continent, particularly in Lincolnshire, Norfolk and Kent. It should be observed also (although small-scale maps fail to present the facts clearly) that the banks of the three great highway rivers, the Severn, Trent and Thames, were seldom regarded by mill builders as suitable sites for milling operations. The lower waters of the Severn, for example, were free of mills as far as Tewkesbury. On the Trent, the first three mill wheels appear at Grassthorpe thirty or forty miles from its junction with the Humber. On the Thames the first mill upstream appeared possibly at Staines but more probably at Basildon.

On the tributaries of these three main waterways, however, and on the numberless smaller, shorter streams fringing the coast line of England, east, south, and west, the story was different. Every northern affluent of the Thames, with the exception of an occasional river in Essex, bore at least a few mills upon its banks. The group of water-courses flowing southeast from the Cotswolds, including the Cherwell, Ray, Glyme, Dorn, Evenlode, Sars Brook, Windrush, Sherborne, Dikler, Leach, Coln and Char turned many wheels. (FIGS. 1 and 4.) South of the Thames, the short streams draining the North Downs in Kent carried the majority of mills in that rather densely milled county, while further west the Kennet, Lambourne, Enborne, and Loddon were thickly milled. The water power afforded by the complex of river

[21] A number of mills are mentioned as existing on manors which now seem to be unwatered. It has been suggested that these may have been operated by horse power. It is more likely that changes in rainfall have occurred leading to the drying up of formerly water-filled valleys. G. M. Meyer, ' Early water mills in relation to changes in rainfall in East Kent', *Quarterly Journal of the Royal Meterorological Society* (1927), LIII, 407–19.

[22] R. Pearse Chope, ' Domesday mills in Devon and Cornwall', *Devon and Cornwall Notes and Queries* (1922), XII, 21–23.

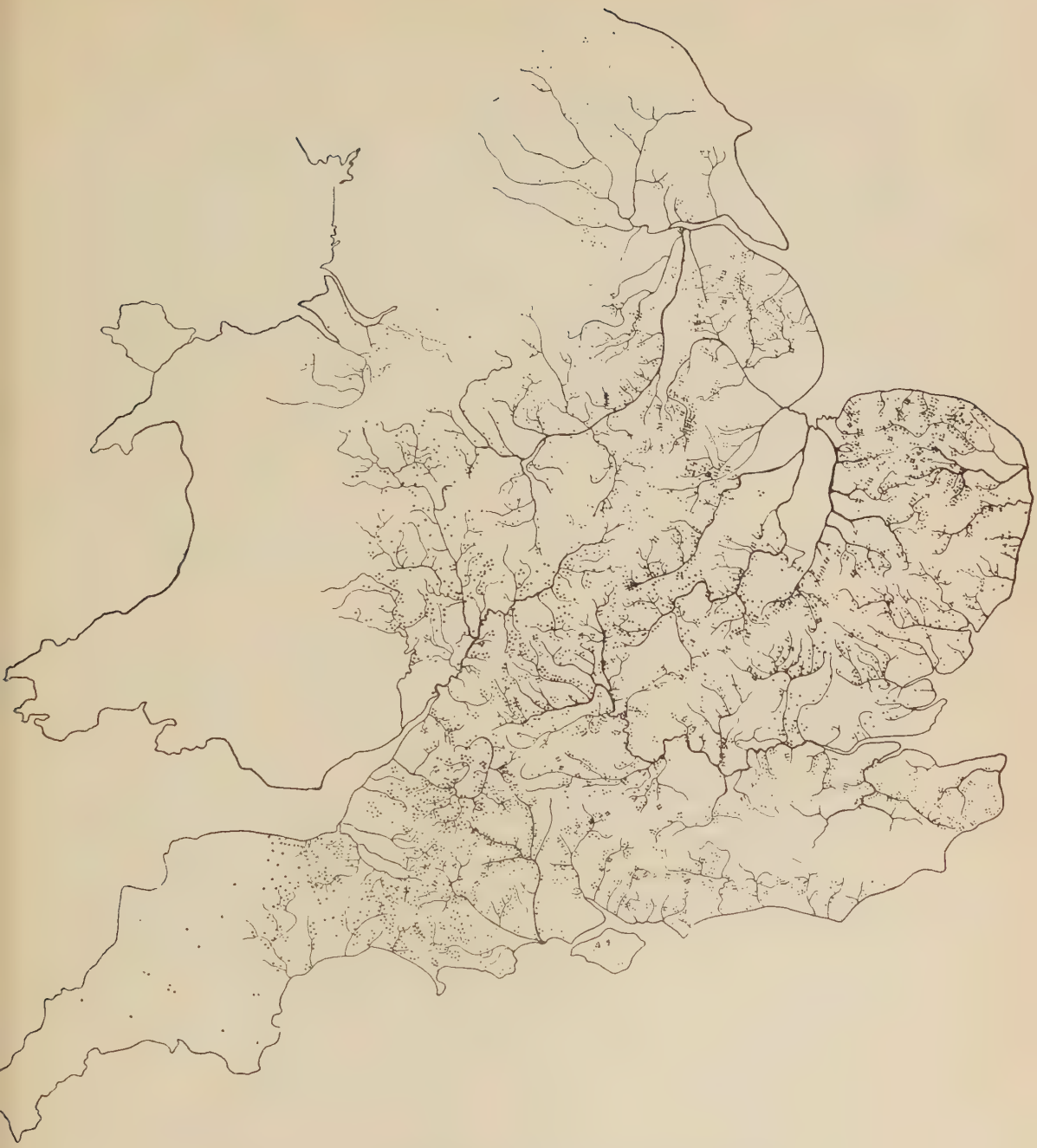


FIG. 1. SKETCH MAP OF DOMESDAY WATER MILLS AND RIVER SYSTEMS

systems centring on the Fenland and Wash was similarly utilized, particularly in Cambridgeshire, Bedford and Northampton. Every coastal stream from the Wash to Lyme Bay, with the exception of those flowing through marshland or dense forest, was crowded with mills, separated in innumerable cases by less than a mile. In thickly milled valleys, such as the Vale of Wylye, thirty mills existed along approximately ten miles of water.[23] (FIG. 3). In short, when William invaded England, the water mill was a commonplace on the great English plain. Assuming, as seems possible, that the county boundaries were the same in 1086 as at the present day,[24] the county of Norfolk contained 537, Lincoln 436, Wiltshire 390, Somerset 351, Kent 334, Hampshire 320, Gloucester 250, Dorset 226, Suffolk 218, Northampton 206 and Essex 203. The greatest density of mills occurred in eastern counties facing the continent and around the central uplands in Wiltshire, Somerset, Gloucester, Dorset and Hampshire. Mills were absent or infrequent only on the Weald, in the Fens, in New Forest, on the moors of western Devon and Cornwall, and on the rivers draining the counties north of the Cotswolds, Northampton uplands and Lincoln Edge.

MANORIAL DISTRIBUTION

A mill was the unfailing mark of a manor, but in spite of their great number, not every manor had a mill.[25] Of the 9250 manors or vills recorded in Domesday Book[26] only about one-third, or 3463, were the sites of mills.

Under the agricultural system reflected in the Inquest it was obviously customary to erect but one mill, or at most two, on a site. Of the 3463 milled manors, 2220 or almost two-thirds were of the single mill type, while 2957, or 85 per cent, possessed two mills or less. It may be assumed, therefore, that one or two mills of even slight power and indifferent regularity were regarded as adequate to grind the corn grown on the majority of milled manors and on the near-by unmilled manors.

It is to be observed, however, that in a significant number of vills, namely 506, clusters of water mills existed ranging in size from three to fifteen (FIG. 2). In Hampshire and Wiltshire, for example, on the

[23] In Scotland, where a similar type of mill abounded, no fewer than fifty-one were enumerated within a radius of about eight miles (O'Reilly, *op. cit.*, 82).

[24] F. M. Stenton, *William the Conqueror and the rule of the Normans* (1908), 489.

[25] J. H. Round, 'Domesday Book and other kindred records', in the *Victoria History of Devonshire* (1906), I, 399.

[26] H. de B. Gibbins, *Industry in England* (1920), 70.

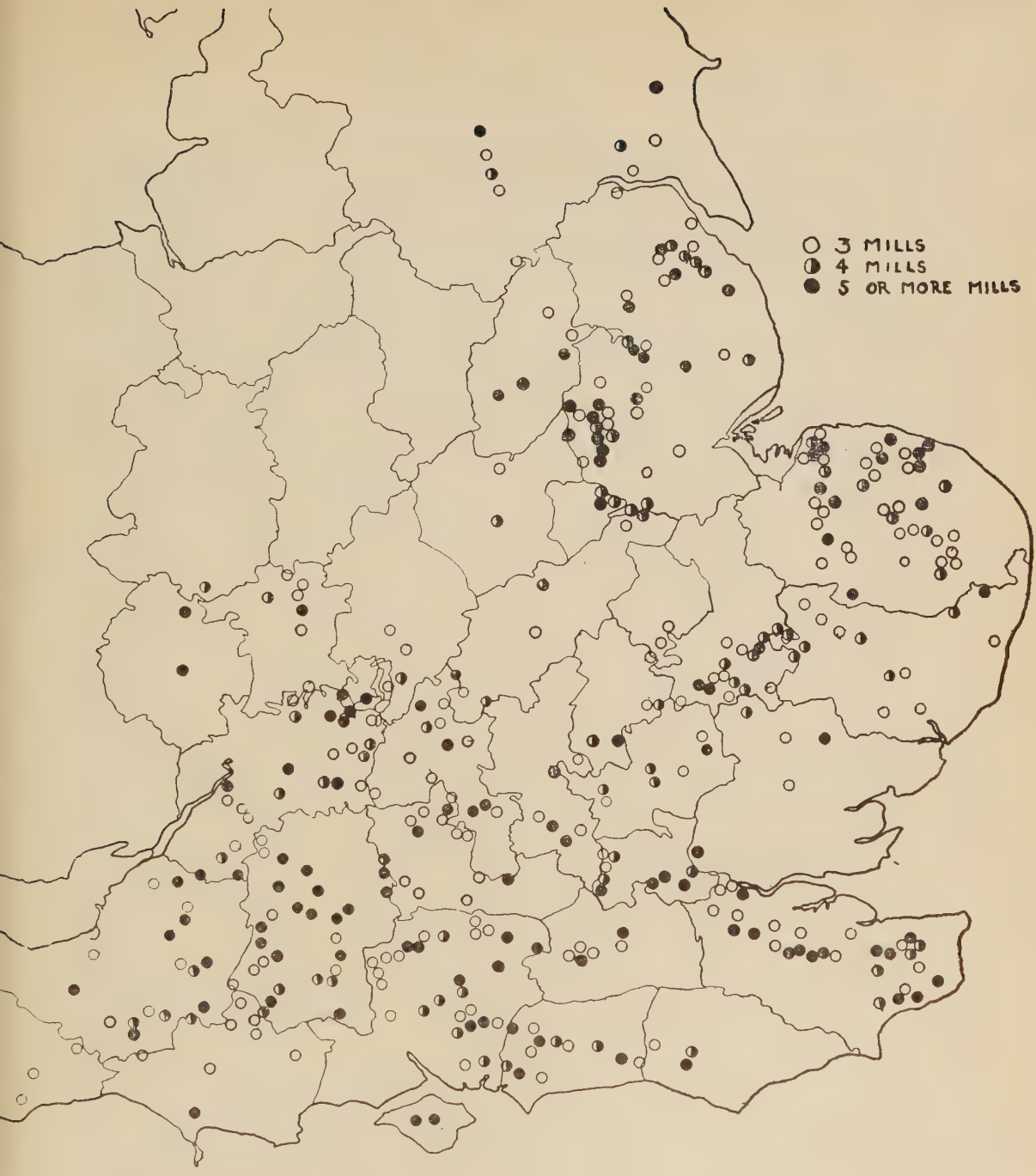


Fig. 2. COUNTY DISTRIBUTION OF WATER MILL CLUSTERS

rivers or brooks draining Salisbury Plain and the North Downs, many vills in the narrow sheltered valleys had from 4 to 9 mills (FIG. 3). The little Meon turned the wheels of 7 at East Meon and 4 at Soberton. Old Alresford on the Itchen was the site of 9 mills. On the Test, the manors of Overton, Hurstbourne Priors, Andover and Abbots Ann each possessed 3 or 4 or 5. On the Wiltshire Avon and its tributaries, the mills of Donhead St. Andrew numbered 8 ; Downton, Pewsey, and Warminster 7 ; Westbury and Potterne 6. Similar clusters existed on the headwaters of the Witham in Lincolnshire, on the short streams of Norfolk, on Thames tributaries in the Cotswolds (FIG. 4), and along the Rother at the foot of the South Downs. Kent contained 17 or more manors bearing 5 or more mills ; Norfolk and Lincoln 12 ; Somerset 9 ; Hampshire 8 and Gloucester 7 ; the remaining milled counties 5 or less. One manor in Norfolk was the site of 9 mills, while on 11 others 5 to 8 were maintained. In Lincoln, one manor possessed 12 mills, while 11 others employed from 5 to 8. The most impressive concentration of milling facilities appeared in Kent, at the foot of the North Downs, where 26 per cent of the mills occurred in clusters of three or more, and one manorial tenant in Westgate, Canterbury, enjoyed the profits from 15.

These local concentrations appeared for the most part in regions of greatest mill density, around the central highland and in the east facing the continent. They also underline and emphasize the radial distribution of water-power utilization. Contrary to expectation, however, mill clusters were not associated with the more or less urban agglomerations of eleventh-century population or with the borough form of social organization. Of the 103 Domesday communities, styled boroughs or stated to contain burgesses, only 16 possessed mill groupings of this type.[27] Mill clusters occurred almost without exception in the smaller rural communities and on the headwaters of smaller watercourses. The only counties in which no mill clusters of more than two appear were four in number, Derbyshire, Staffordshire, Cheshire, and Cornwall, all of which lay on the northern or western boundaries of the water mill distribution.

Neither Domesday Book, nor the critical investigation to which it has been subjected, suggests an explanation for this striking variation from the common practice of maintaining a single mill, or at most two, on a manor site. It is a temptation to view the initiative which led to the

[27] Adolphus Ballard, *Domesday boroughs* (1904), 9–10.

FIG. 3. WATER MILL CLUSTERS ON THE STOUR, HAMPSHIRE AVON, WYLYE, TEST AND ITCHEN RIVERS

erection of large groups of mills on single manors as something akin to capitalist enterprise in a pre-capitalist society. Without further evidence, however, this explanation is somewhat gratuitous. When the geographical positions of mill concentrations are examined it seems more likely that they were erected in response to the local coincidence of restricted water power and rural population density. This was patently the case in populous Kent where the rivers were few and short and the arable area reduced by the encroachment of the Weald. It is also suggested in Cambridgeshire, where the marshes in the northern part of the county forced settlement into the river valleys of the south. In this connexion it is of interest to note that, had the 130 mill sites of Kent borne one mill only, each would have had to serve over 90 households. When, however, the available waters were made to turn 3 or 4 or even 15 mills at a site, each mill served an average of 36 households. Similar figures emerge for Norfolk, Gloucester, Cambridge, Hampshire and other counties conspicuous for density of population, limited water power and mill concentration.

Ratio of Mills to Population

Unfortunately the Domesday enumeration of the population of eleventh-century England is much less complete than its treatment of fiscal matters. Indeed, it was not the purpose of the Inquest to make what is now called a census of the whole population. In reporting 287,045 tenants and occupiers of land by class and county, however, and thus leaving us to suppose, in Maitland's phrase, that ' each of these persons is or may be the head of a household ' of five,[28] a total population is suggested of approximately 1,400,000 souls.[29] When these figures are compared with water mill figures, as classified by county (Table 1), it becomes plain that, while for England as a whole each mill may be presumed to have served an average of 50 households or ' families ', the ratio of mills to population actually varied widely from county to county and from region to region. In Wiltshire, on the one hand, with 10,150 households, there were 390 mills, or one to every 26 ' families '. In Norfolk, on the other, with 537 mills and 27,078 households, or almost three times the population of Wiltshire, there was only one mill to every 50 households. With the exception of Norfolk and Lincolnshire, the counties containing the largest number of people were thus not necessarily the counties containing the largest number of

[28] F. W. Maitland, *Domesday Book and beyond* (1897), 17, 437.

[29] Gibbins, *op. cit.*, 66.

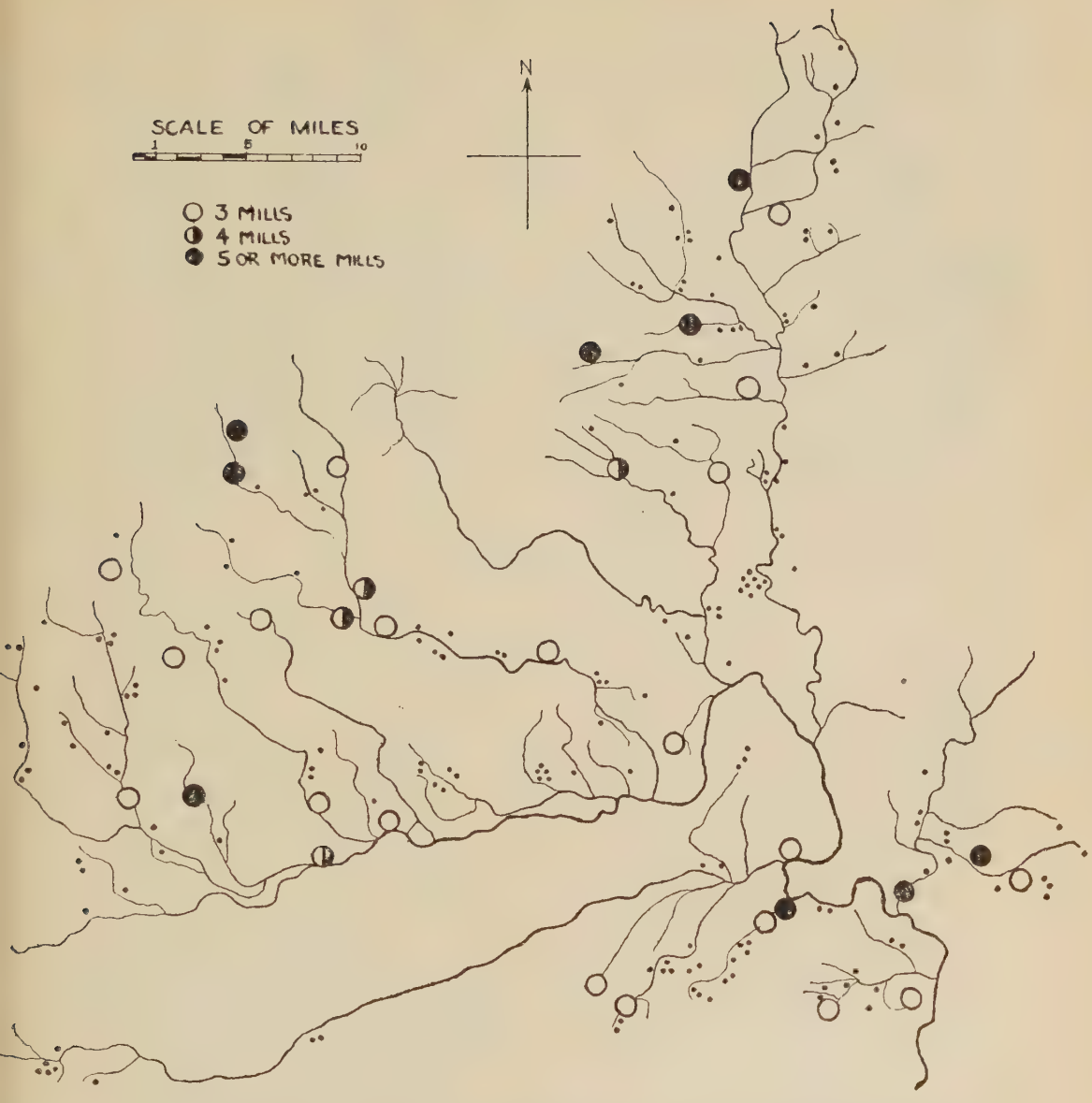


FIG. 4. WATER MILL CLUSTERS ON THE TRIBUTARIES OF THE THAMES IN THE COTSWOLDS

mills. Suffolk, for example, stands third in the number of households but ninth in the number of mills. Or, stating the situation in another way, the counties containing the greatest number of mills were not necessarily those most adequately milled. It has been asserted that the average population of an eleventh-century village was about 150 persons[30] or 30 households, an estimate which, even in Wiltshire with one mill to 26 ' families ', would leave six households per mill unprovided with access to milling facilities. But if, for the sake of argument, the ratio of one mill to 26 ' families ', which obtained in Wiltshire, be accepted as a measure of the number of mills just necessary to meet the needs of a given population without recourse to querns, then Norfolk and Lincoln, with a somewhat greater number of mills and a far greater number of households, were only half as well served with milling facilities. In fact, if the maps A, C, and D in FIG. 5 be compared, it will be seen that, while the density of population decreases as we pass from east to west, the density of mills (Norfolk and Lincoln excepted) increases, and hence the adequacy of milling facilities. The areas of greatest mill density and greatest population density do not coincide except around the central highland and in Kent. Although the five populous eastern counties of Norfolk, Suffolk, Essex, Lincoln and Kent possessed 35 per cent of the estimated population of England and 30 per cent of the mills, the five western counties of Gloucester, Wiltshire, Somerset, Dorset and Hampshire contained 17 per cent of the population and 27 per cent of the mills. Such a comparison suggests that had Norfolk, as the most populous county, been as adequately milled as Wiltshire, it would have possessed 1100 mills instead of 537. Had Domesday England, as a whole, been as adequately milled as Wiltshire, there would have been 12,000 mills instead of 5624.

There can be no doubt that, in spite of the widespread acceptance of water-driven mill-wheels and the application of the right of multure, some use of querns or saddlestones remained a necessity in all but a few communities. Indeed, in the presence of a shortage of mills, the insistent effort to compel tenants to grind at the lord's mill is a little hard to understand. This shortage was marked in the far west and far east. In Cornwall with one mill to every 1087 households, and in Devonshire with one to every 200, the use of the quern must have been practically universal. To a lesser degree, the same statement can be made for the eastern counties of Suffolk and Essex, with 93 and 79

[30] Gibbins, *op. cit.*, 81 and note 3. See also Maitland, *op. cit.*, 17–20.

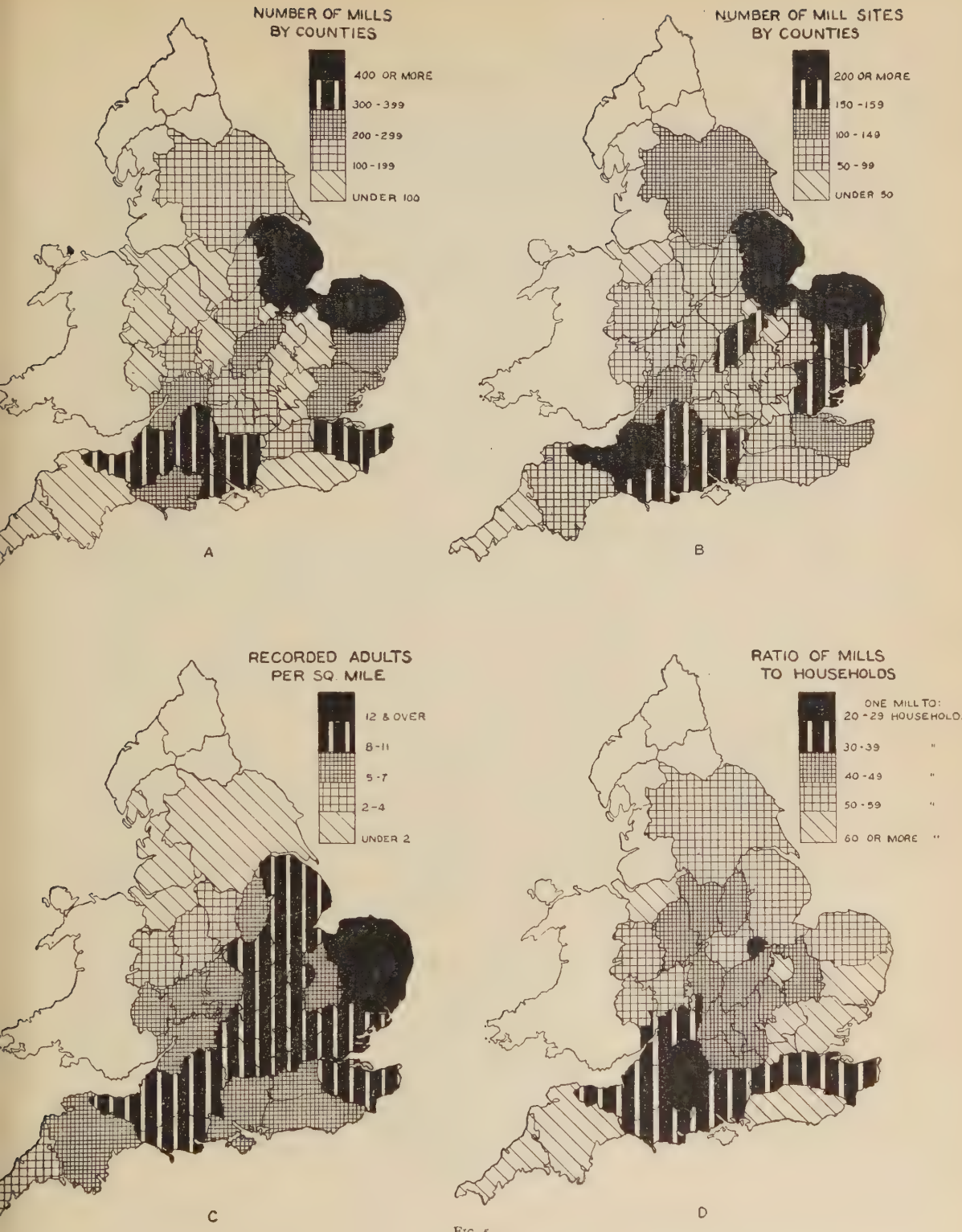

NUMBER OF MILLS
BY COUNTIES
400 OR MORE
300 - 399
200 - 299
100 - 199
UNDER 100
A
NUMBER OF MILL SITES
BY COUNTIES
200 OR MORE
150 - 159
100 - 149
50 - 99
UNDER 50
B
RECORDED ADULTS
PER SQ. MILE
12 & OVER
8 - 11
5 - 7
2 - 4
UNDER 2
C
RATIO OF MILLS
TO HOUSEHOLDS
ONE MILL TO:
20 - 29 HOUSEHOLDS
30 - 39 "
40 - 49 "
50 - 59 "
60 OR MORE "
D

FIG. 5

respectively. The only rural groups which took definite advantage of the new device and were by that token most hospitable to cultural change in Saxon England, were those which lived around the central uplands and in Surrey and Kent.

Whether or not these regions in which querns were least necessary were also the areas in which the water mill was first introduced is a question of some interest, but one for which dated history has no answer. Many anthropologists, confronted by such a problem, would be tempted to interpret geographical density of trait-distribution as an indication of prolonged trait use, and thus suggest that the water mill first appeared in Lincolnshire and Norfolk from a possible continental source. Or observing a similar density in the Salisbury Plain region, they might surmise that the mill had been twice introduced, once from the northeast and again from the south, each introduction leading to a diffusion into surrounding areas.

But the age and area hypothesis, as applied to cultural facts, has never received adequate historical verification and conceals many pitfalls for the unwary. If then it be deemed necessary to locate the region in eleventh-century England in which the water mill had been longest known or earliest employed, it would seem wiser to appeal to an argument based upon the distributions of mill clusters and jointly owned mills. Assuming with respect to mill clusters that a second mill would be built on a site only after a first one had been found useful, and that therefore two-mill sites were older than one-mill sites, and assuming further that sites bearing 5 to 15 mills were far older than sites bearing a lesser number, the conclusion is reached that the water mill appeared first in Kent, with Lincolnshire and Norfolk following soon after. (Table 2). Or assuming jointly owned (or fractionally stated) mills to be the outcome of division by inheritance, and the areas bearing the greater number of such mills to be those in which a relatively long period of division had existed, Norfolk, Lincolnshire and Wiltshire again loom very large.

Indeed, all arguments agree in pointing to the Salisbury Plain region as one in which the water-driven mill had been employed long enough to lead to marked indications of capitalist enterprise and inherited ownership. While for similar reasons the eastern counties of Kent, Lincolnshire and Norfolk appear to have been equally responsive to cultural change in the period just antecedent to the Norman Conquest. Our knowledge of the process of change in human activities is too rudimentary to afford an explanation of why these areas were the

first (if they were) to introduce the water mill, or why a useful device should have been welcomed early by some groups of rural folk and rejected by others. We have no way of knowing yet why an element of advanced culture was immediately incorporated into the life of one agricultural community and resisted by an adjoining one. It is suggested, however, that this difference in initiative among human beings is of crucial importance in accounting for technological advancement as well as social ' progress '.

TABLES showing the number of water mills, heads of households, households per mill, and sites in the counties of Domesday England are printed on pages 278–9.

TABLE 1

Showing the number of water mills, heads of households, and households per mill in the counties of Domesday England

Counties	Number of Mills			Number of	
	Unidentified	Identified	Total	Heads of Households*	Households per Mill
Norfolk		537	537	27,087	50·4
Lincolnshire ..	24	412	436	25,305	58·0
Wiltshire	10	380	390	10,150	26·0
Somerset		351	351	13,764	39·2
Kent	20	314	334	12,205	36·5
Hampshire		320	320	10,373	32·4
Gloucester	27	223	250	8,366	33·4
Dorset	2	224	226	7,807	34·9
Suffolk	12	206	218	20,491	93·9
Northampton ..		206	206	8,441	40·9
Essex	3	200	203	16,060	79·1
Sussex	6	158	164	10,410	63·4
Oxford	6	156	162	6,775	41·7
Yorkshire		145	145	8,055	54·8
Berkshire		135	135	6,324	46·7
Nottingham ..		128	128	5,686	44·4
Cambridge	6	119	125	5,204	41·6
Buckingham ..	1	125	126	5,420	43·3
Leicester		123	123	6,772	54·6
Surrey		120	120	4,383	35·6
Warwick	3	114	117	6,574	57·6
Hereford	2	96	98	5,368	54·1
Worcester		97	97	4,625	46·7
Bedford		93	93	3,875	42·6
Shropshire		88	88	5,080	56·5
Devonshire ..		87	87	17,434	200·1
Hertford		82	82	4,927	60·0
Derby		68	68	3,041	43·2
Stafford	9	59	68	3,178	46·7
Middlesex		36	36	2,302	63·9
Rutland		31	31	862	27·8
Huntingdon ..		31	31	2,914	94·0
Cheshire	1	23	24	2,349	97·8
Cornwall		5	5	5,438	1087·6
Total	132	5,492	5,624	287,045	

*Sir Henry Ellis, as cited in H. de B. Gibbins, *Industry in England* (1920), 67.

DOMESDAY WATER MILLS

TABLE 2

Showing the number of identifiable water mill sites and the number of mills per site in the counties of Domesday England

Counties	Number of Mill Sites Bearing Mills															Total Sites
	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	
Norfolk ..	160	93	29	8	6	2	1	1	1							301
Somerset ..	162	51	7	3	4	2	2		1							232
Lincolnshire	114	56	15	16	3	4	3	1				1				213
Wiltshire ..	111	52	8	5	2	3	3	4	2	1		1				192
Hampshire ..	98	42	20	7	1		2	2	1							173
Dorset ..	119	31	9	2				1								162
Northampton	105	39	5	2												151
Suffolk ..	115	24	6	5	1											151
Essex ..	116	29	3	1	1			1								151
Kent ..	62	33	12	5	6	4		2	1	2	1				1	129
Gloucester ..	58	22	17	6	3		1	1		1						109
Yorkshire ..	86	11	6	2				1								106
Oxford ..	59	23	7	1	4	1										95
Sussex ..	60	13	6	4	2			1	1		1					88
Warwick ..	68	16	2	2												88
Buckingham	62	15	5			1		1								84
Leicester ..	59	20	2	1			2									84
Nottingham ..	51	24	2	1	1	1										80
Berkshire ..	49	19	9		1	1	1									80
Shropshire ..	69	8	1													78
Devonshire ..	62	8	3													73
Hereford ..	56	10	1		1			1								69
Worcester ..	48	8	8	1	1							1				67
Surrey ..	41	16	5		1	2	1	1								67
Derby ..	50	9														59
Bedford ..	35	19	2	2		1										59
Stafford ..	43	8														51
Hertford ..	32	12	3	3	1											51
Cambridge ..	20	10	9	8	1		1	1								50
Cheshire ..	19	2														21
Huntingdon ..	11	7	2													20
Rutland ..	11	3		2		1										17
Middlesex ..	6	3	2	3		1										15
Cornwall ..	3	1														4
Total Identifiable Sites ..	2,220	737	206	90	40	24	17	19	7	4	2	3			1	3,370
Unidentifiable Sites ..																93
Total Sites ..																3,463

Air Reconnaissance of Roman Scotland

by O. G. S. Crawford

ON 6 June I started from Southampton to carry out an investigation of the Roman roads and sites in Scotland from the air. For several years, as part of my official duties, I have been examining these on foot, in pursuance of a plan to publish a third edition of the Ordnance Survey Map of Roman Britain.* It is intended to prepare the way for this map (which may be published on a slightly larger scale—10 miles instead of 16 miles to the inch) by the publication of maps of special regions on the scale of 4 miles to the inch. The drawing of the first two of these (Scotland, Sheet 3 [Forth and Tay], and Sheet 1 [The Border]) had been finished; but many doubtful points remained even after intensive field-work, and it seemed probable that a short air reconnaissance under favourable conditions would solve some of them. This opinion was amply justified by results. About a dozen new Roman sites (including as 'sites' new stretches of Roman road) were discovered; about 50 new sites in all, including many native forts, were placed on the map; and valuable results of a general character were obtained.**

June was chosen because then crop-sites, revealed by growing corn, are seen at their best. Last June was a particularly favourable month for the work, for a long drought had burnt up the grass leaving untouched the silted-up ditches, pits and post-holes which looked as if they has been painted in vivid green on a brown canvas. We were fortunate in catching the last week of this drought; the weather broke on the day that we flew south. We were thus able to observe and photograph the sites under the best possible conditions. Furthermore, work of this kind cannot be carried out continuously in a mountainous country like Scotland except during a period when clouds are high or absent. It is not often that one can fly round the Devil's Beeftub and Hartfell day after day under such perfect conditions.

The work was made possible by the enterprise and generosity of Mr Geoffrey Alington, of Air Touring; many of the new sites were in fact seen first by Mr Alington, who flew the aeroplane (a Puss-Moth)

* See note on page 292.

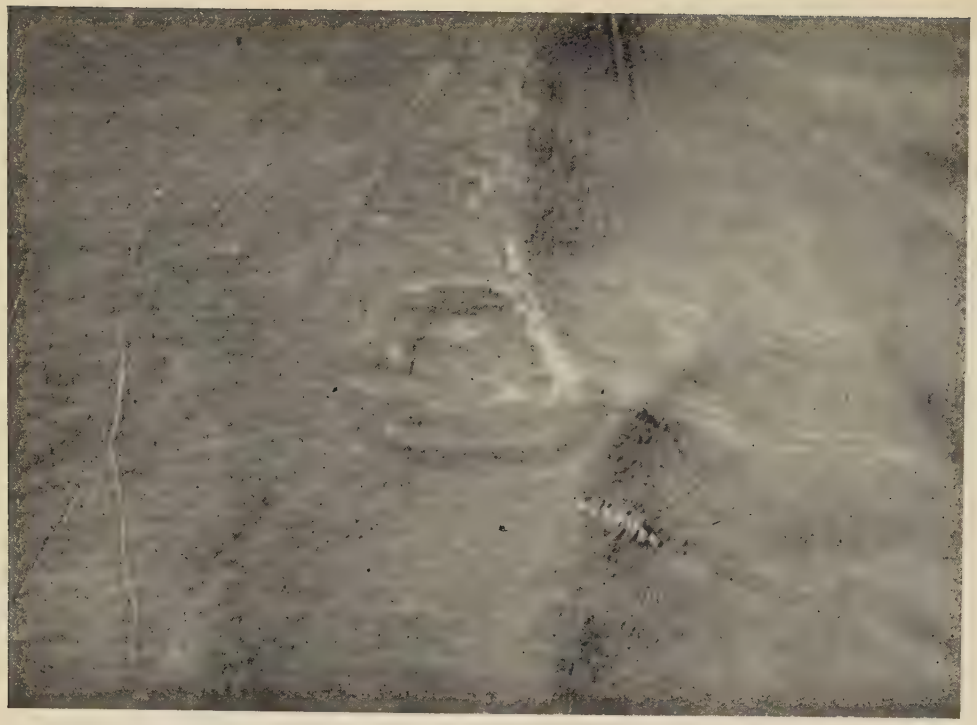
** My own observations have been made over, for publication in greater detail, to Mr K. St. Joseph, who is undertaking trial excavations for the Glasgow Archaeological Society at a number of the sites, as part of this Society's scheme of investigation of Roman works in southwest Scotland.

PLATE I

EDINBURGH FROM THE NORTHWEST
Ph. Geoffrey Allington

facing p. 280

PLATE II



A, ROMAN FORTLET, DALMAKETHAR (*see* p. 282)
Ph. Geoffrey Alington

B, ROMAN FORTLET, REDSHAW BURN (*see* pp. 281, 282)
The Roman road is the upper one, forking from the 18th cent. road
Ph. Geoffrey Alington

A, ROMAN ROAD NORTH OF BUSHEL BECK NEAR MOFFAT, SHOWING QUARRY-PITS (*see* p. 283)
Ph. Geoffrey Alington

B, SOUTHWEST CORNER OF ROMAN MARCHING-CAMP, TORWOOD, WEST OF LOCKERBIE, REVEALED BY PARCHING (*see* p. 284)
Ph. Geoffrey Alington

PLATE IV

A, NATIVE FORT, WARD LAW, CAERLAVEROCK, DUMFRIES, IN TREES; BEYOND, A PROBABLE ROMAN (SQUARE) FORT FAINTLY VISIBLE (*see* p. 284)

Ph. Geoffrey Alington

B, ROMAN ROAD NORTH OF LOCHMABEN REVEALED BY PARCHING (WHITE LINE=BROWN ON GROUND) ROUND NATIVE FORT OF WOODY CASTLE IN FOREGROUND (*see* p. 284)

Ph. Geoffrey Alington

PLATE V

A, ROMAN MARCHING-CAMP (?) AND NATIVE PROMONTORY-FORT (?) GALLABERRY, NORTH OF CARZIELD, DUMFRIES; REVEALED BY PARCHING (*see* p. 285)
Ph. Geoffrey Alington

B, ROMAN FORT, BIRRENS, WITH ANOTHER FORT, AS CROP-SITE IN FOREGROUND (*see* pp. 285–6)
Ph. Geoffrey Alington

PLATE VI

A, ROMAN SIEGE-FORT ON SOUTHEAST SLOPE OF BURNSWARK, DUMFRIESSHIRE (*see* p. 286)
Ph. Geoffrey Alington

B, ROMAN SIEGE-FORT ON NORTHWEST SLOPE OF BURNSWARK (*see* p. 286)
Ph. Geoffrey Alington

A, MEDIEVAL STRIP-LYNCHETS ON SHAW HILL, ABOVE CULTER, LANARKSHIRE (*see* pp. 289, 290)
Ph. Geoffrey Alington

B, MEDIEVAL STRIP-LYNCHETS ON THE EAST SLOPE OF ARTHUR'S SEAT, ABOVE DUNSAPIE LOCK, EDINBURGH:
Note the corresponding group of riggs and furrows immediately below (*see* pp. 289, 290)
Ph. Geoffrey Alington

and took all the photographs. Without his cooperation the results here described could not have been obtained.

We left Southampton at 11.15 a.m. on 6 June 1939, and landed on Lanark race-course at a quarter to four. The weather was perfect, and visibility excellent except where obscured by the smoke haze of the midlands. Soon after leaving Southampton I observed on the horizon ahead of us a plume of high cirrus cloud. I watched it gradually get higher; but it was not overhead until we were well north of the latitude of Liverpool. When first seen, therefore, it must have been about 180 miles distant. (We were flying at about 2000 feet or lower). We flew along the Lancashire coast and over the Cumberland mountains, crossing the Solway to Annan, where we reached familiar ground. From here up Annandale and down the Clyde we followed the Roman road and made the first discoveries. I observed a small unrecorded native fort on Woodcock Air (Dumfr. 57 S.E.), in a clearing on the top of the hill, and another on the top of Quhytewoollen Hill (Dumfr. 43 S.W.) where I had already expected to find one. I saw another on Quothquan Law (Lan. 33 S.E.), divided into two parts by an earthen rampart; from the ground I had suspected the existence of such a fort, but from some points of view the hill-top seemed quite devoid of any fortification. (One of the advantages of air reconnaissance is that it avoids the time and labour of climbing likely-looking hills that may prove barren of remains at the top !). After our arrival at Lanark we decided to go up again and confirm these discoveries. The evening was cloudless and the brilliant low light perfect for observation. We flew back to the Devil's Beeftub above Moffat and then up the Potrail Water to Durisdeer, confirming the existence of two Roman fortlets discovered on the way up and finding another probable one. These were at Bushel Beck (Dumfries 9 S.W.) and Redshaw Burn (Lanark 51 N.W.; PLATE II B); the doubtful one was beside the Durisdeer road at Allan Cleuch (Lanark 53 S.W.). I also observed the course of the Roman roads, confirming my field-work everywhere, and adding a portion north of Crawford.

These fortlets are a recent addition to knowledge, at any rate as a normal accompaniment of Roman roads. They are partly due to air observation, one of them being that at Barrock Fell (Cumberland 31 S.W.), discovered and photographed from the air by Group Captain Insall, V.C., in 1930. This was excavated by Professor R. G. Collingwood and Mr R. E. Porter (*Trans. Cumb. and Westmorland Arch. Soc.*, 1931, N.S. XXXI, 111–18. The existence of such fortlets has of course been suspected since the time of General Roy, whose site at Tassiesholm

belongs to this group. Last year another was identified at Dalmakethar (Dumf. 33 NE, PLATE II A) by Mr K. St. Joseph in the course of his field-work done, in a voluntary capacity, for the Ordnance Survey, while surveying the Roman roads and sites on the Border Sheet; and the fort at Durisdeer, for which a Roman origin had often been claimed, was proved to be in fact Roman by Mr J. Clarke's excavations.

The redoubt in the NW corner of the Roman fort on Burnswark (SE slope, PLATE VI A) is another of the same kind. Later on we found yet another near the Beeftub, at Rowantree Grains. There are thus five examples of this type of fortlet along the Roman road between the marching-camps at Little Clyde and Torwood Muir. All are placed close beside the Roman road, with the single entrance facing it. It is not yet possible to say whether they were placed at any stated intervals; Redshaw and Rowantree are less than three-quarters of a mile, and the others several miles, apart. But there may be many more to be discovered even on this road; and it seems reasonably certain that they will be found on other roads within the military area of Roman Britain, and perhaps elsewhere. The one thing that can be said with certainty is that some of them are placed at the crossing-points of ravines, probably to guard a wooden bridge. Their shape is shown best by Redshaw and Dalmakethar (PLATE II A); it is that of a small rectangle with rounded corners, or of a flattened oval. In proportion to the area enclosed the ramparts are rather wide, and that is one of the features which enables them to be recognized when seen from above. They have, moreover, when thus seen, the massive regularity characteristic of all Roman work. These features enable one to distinguish Roman fortlets from native works, whether prehistoric, medieval or later. Several of the native forts, particularly on the hills above Annandale, closely resemble these Roman works, and may have been influenced by them. One such on Broomhill bank, for instance, is less than two miles to the E.S.E. of Dalmakethar.

We flew several times along the Roman road through Annandale and the Upper Clyde, and each time we made fresh discoveries. On one occasion we found a small round signal station of the Gask type at the top of the pass near the Beeftub. It consisted of a ditch with a break (for entrance) facing the road, We thought we saw remains of others along the ridge further south (and elsewhere), but these must be tested on the ground. It is easy to confuse these slight relics with the remains of sheep folds; but the latter consist of a rather broad round *bank*, usually without any break in the circumference.

The stretch of Roman road from the Beeftub Pass to Moffat golf-links (5 miles) is one of the best and most perfect in the kingdom. The ground over which it passes is moorland that has never been cultivated. There are places, especially at the northern end, where it is almost lost in bog ; but there are no other considerable breaks in its continuity. Its Roman origin was never seriously questioned by the earlier field archaeologists ; and their opinion was confirmed by me when I walked along it in 1924, and restored it as such to the one-inch Ordnance Map. Since then I have walked along it again throughout its whole length from its northern known limit* to Burnswark. The quarry-pits from which materials for the causeway were dug accompany it on either side throughout, and are well seen on PLATE III A. These quarry pits are in fact valuable aids to recovering the course of the road, when the causeway itself is not easily traceable. (I was thus put on the right track by a row of quarry-pits on the hill above Walstone, N.E. of Carlops ; air-observation revealed the quarry-pits very clearly, together with the causeway between them, which was barely visible on the ground). The later (18th century) road-makers followed a different procedure. They obtained the materials for their road-causeways not from small pits dug in the flat ground beside the road but from much larger quarries dug in the side of the hill where the road ran as a shelf along the slope. These later hillside quarries accompany the 18th century successor of the Roman road and medieval track over the same route by the Beeftub. Here they are fairly regularly spaced at intervals of about half a mile. The presence of such hillside quarries beside a causeway road is a warning that the road in question is not, in its present form, at any rate, to be accepted as Roman.

* North of Little Clyde Mr St. Joseph traced its course in 1938 through Upper Clydesdale to Crawford. Thence it goes over the Raggengill pass to join the Clyde valley again near Coldchapel. At Wandel it falls into the modern road with which it seems to coincide to Candeyburn, north of Biggar. There is a perfect stretch of it south of Melbourne, on the moor east of the modern road. I found a mile of it between Dolphinton and Hardgatehead from the air. There is another well-preserved section north of West Linton. In both these last instances it is plainly distinct from the metalled (18th century ?) road which runs more or less parallel to it at a considerable distance. It is last seen for certain, with quarry-pits, on the hill above Walstone. I discovered it last winter. Mr St. Joseph has referred me to Maitland (1, 193) who says it reached the ' eastern end of the Pentland Hills, in the neighbourhood of which it is to be seen pointing to the station at Cramund '. The Annandale road thus ran towards Edinburgh rather than Glasgow, as has previously been supposed, and never went to the fort at Corbiehall near Carstairs ; though of course there may have been a branch thither separating off perhaps near Biggar.

Later on we reconnoitred some other Roman sites in the neighbourhood of Lockerbie and Dumfries. We saw the missing SW corner of the marching-camp at Torwood (PLATE III B) which eluded Roy. It appeared as a bright green mark, like an L with round corner, in a field that had been parched brown by the drought. Recently both Mr St. Joseph and I had found the *tutuli* guarding gates in its west side, now hidden in the wood. Suitable conditions would doubtless reveal the ditches of its other sides, which pass through fields now in cultivation.

Earlier in the year I had visited the site of a reputed Roman fort at Ward Law (PLATE IV A), above the Castle of Caerlaverock, 7 miles south of Dumfries. This was seen in 1772 by Pennant, who described it as ' faint vestiges of a Roman camp, now much ploughed up ' (Tour I, 1790, 108 : he is wrong in saying that it lay on the *south* side of the hill). The mound was still sufficiently plain last winter for me to be able to trace it all round, and even to photograph it on the ground. Its outline, fortunately, was marked by a broken line on the Ordnance Map in 1856, when doubtless it was more evident than now. It is nearly square in plan and has the appearance of Roman work ; but it cannot be marked as Roman on our map until it has been tested by excavation. When flying over it I could hardly see it, for the crops, if any, must be unsuitable for showing up the ditch ; but on the photograph the ghostly outline is faintly visible above the clump (which hides a native fort). The wall running north from the clump divides it approximately in half. Round the outside of the clump on the west and south runs a belt of bright green grass revealing the outermost ditch of the native fort ; and south from it is a long row of tiny black (=green) dots representing pits, probably post-holes.

There are persistent accounts in the old writers of a Roman road running westwards north of Lochmaben by Woody Castle. It is marked as an ' Old Road ' on the Ordnance Maps. I had already investigated this on the ground during the winter, and carried it a little further west ; it seemed to be Roman, but I was not prepared for so startling a confirmation as that which I now saw, and which is shown on PLATE IV B. The straight white band pointing directly at the observer is a far more authentic record than the almost imperceptible fold which was all I could see on the ground. The photograph proves, therefore, not so much the value of air observation (though it does this) as the reliability of careful ground observation, even when the visible remains are of the slightest. The road continues along a curiously zigzag course westwards to Broomrig ; but, though we got a hint or two of

its possible continuation (to be tested on the ground later), all our efforts to trace it definitely were unsuccessful. The hints we got were too slight for mention, but they are consistent with the old accounts which took it by Murder Loch. Whatever its exact route the road must surely have been making for Carzield or its neighbourhood; for at Carzield a Roman fort has recently been excavated by Mr I. A. Richmond and Mr Eric Birley. We had a look at it, and saw the western ditch of the fort, invisible on the ground, plainly revealed as a broad green belt along the steep bank of the plateau. But we could see no remains of the road, as we had hoped, issuing from it, except possibly two dark green curved lines on the eastern slope outside the fort, pointing towards the modern road from Carzield to Kirkton. On the other hand we did discover, close by at Gallaberry, a rectangular enclosure with rounded corners which can hardly be anything but a small Roman marching-camp (PLATE V A). The ditch was revealed by a bright green band contrasting vividly with the toast-coloured grass elsewhere in the field. The region is one of glacial moraines, forming a network of hummocks and hollows, often containing lakelets or bogs; and the tops of the hummocks, and the gravel river-terraces, react strongly to drought. The grass is burnt brown, and any former pits or ditches, having more depth of soil, remain moister and keep their verdure. This sort of country forms an excellent hunting-ground for the air observer during periods of drought. Had we had more time to spare we could certainly have found many more sites. As it was we found two large circles in the bend of the road leading to Brownfield (immediately south of the farm and west of the road), a mile and a half SE of Carzield.

The Roman camp, if such it is, stands on a gravel plateau projecting into the marshy low-lands of the Nith. Across the neck of the promontory runs a curved green line (=a ditch), representing probably a small native fortlet. Close to it, abutting on one end, is a small three-sided enclosure of an unknown character. Along the foot of the bank, in front of the only entrance to the Roman camp, is a long green line extending beyond the width of the camp at either end. Other lines are probably of later (agricultural ?) origin; and a small circle in the foreground, with a gap flanked by two post holes (?) may be a hut enclosure. It seems to stand inside another much larger circle.

The two most celebrated Roman sites in this neighbourhood are Birrens and Burnswark. Birrens has long been recognized as a Roman fort and was excavated in 1895 and 1936–7. It is most interesting to find the outline of another work close beside the well-known fort

(PLATE V B), and on a slightly different orientation. This is undoubtedly the work planned by Roy (pl. xxiv), but the apparent impingement of the two adjacent sides suggests that it cannot be an annexe, as might appear from his plan. One may conjecture that this is the Flavian fort, for the earliest structures on the other site, definitely recognized to belong to a Roman fort, were apparently Hadrianic in age. The three ditches of the new fort are plainly seen from the air, outlined by yellow flowers, which appear as parallel white bands on the photograph. No certain traces of a road issuing from the fort were seen, nor was it found, in spite of constant search, between here and West Gill beyond which to Burnswark its course is known. But the search produced a small native fort, seen as a confused crop-site, midway between Woodlands and Langdyke.

Burnswark is a flat-topped hill 3 miles NW of Birrens. It is a landmark for all the country round ; we could see it from the aerodrome at Carlisle, and I have seen it from the hills near Crawford. From the air it can be seen from even further north, and on our return from Edinburgh on 12 June we saw it from near Biggar and flew straight across country towards it. At close quarters it has a certain grimness that is in keeping with its history[1] ; for it was the 'tribal capital' of the natives who were besieged there by the Romans ; and near it was fought the celebrated battle of Brunanburh (see ANTIQUITY 1937, XI, 283–93). The siege-camps of the Romans are still very well preserved. That on the southeastern slope of the hill is the more perfect of the two (PLATE VI A). It has been suggested that the camp on the NW was unfinished. Our illustration (PLATE VI B), shows however that all four sides were finished. On the south side of the hill, close to the Roman road, is an irregularly shaped earthwork, which has been regarded by some as a Roman redoubt. I have examined it on the ground, and Alington photographed it from the air. Its age and character are still rather uncertain. I am inclined to connect it with the hill-fort on the summit, for there are traces of a track leading apparently from it up the hill. On the other hand, there are the foundations of a rectangular building (black house ?) in a small enclosure between it and the Roman road, with a small cultivation-plot (rigg and furrow), surrounded by a bank, attached.

[1] The bare austerity of its outline is not improved by recent planting on the SE slope, close to the corner of the Roman siege-camp.

Elated by our discoveries in Annandale we decided on the second day of our expedition to fly northwards and see what we could make of the Roman remains in Perthshire and Angus. The region lay rather far from our base, from which it was separated by the Gargunnock Hills, the Ochils and the Sidlaws ; and there are times when these uplands can become unpleasant obstacles to flying. Consequently I was anxious to get this distant area worked off quickly while the weather was settled. We flew first to Perth, where we filled up, thence along Strathmore to Aberdeen. Thence we returned in one flight by Comrie, Glen Artney and Callander. During the flight we inspected all the known Roman forts north of the Antonine Wall except Carpow, and found another further north than any yet known.

First of all I wanted to look at Myot Hill near Denny, which had been connected with the Mæatae[2]. Sure enough on its top I saw the plain outlines of a native fort, hitherto unrecorded. At Stirling we examined and photographed the royal garden called the King's Knot, below the Castle on the west. Here Maitland[3] says a 'Roman station' was destroyed when the gardens were being laid out. The statement seemed very probable, and I had already wondered whether the Knot itself might not have been much older than the gardens, and had marked on my map a ditch which seemed to have no connexion with them. From the air we saw this ditch and another, certainly older than the gardens, which may be part of the remains aforesaid ; but the two ditches seem to have no connexion with each other. It is as certain as anything can be that there was a Roman fort or fortlet to guard the Forth crossing, and this is the obvious place for it. All the earlier accounts, and the existing topography, make the Roman road to Ardoch pass close by, to the west of the Castle Hill.

This is not the place to describe our observations in detail. By far the most important discovery was in part accidental. Years ago I had ringed a site at Cardean near Meigle on my map for investigation, but had forgotten why, except that Roman remains had been found there. When we were flying over it I was just saying to Alington that we had better go on and not circle it, as there was nothing, when we both saw something coming into view. It is a curious fact that even 'flat' sites, like crop sites, are invisible or barely visible from certain angles.

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[2] Watson, *Celtic Place-names of Scotland*, 59.

[3] *History of Scotland*, 1757, I, 194.

What we saw was the broad, unmistakeable rampart of a Roman fort, complete with internal streets, gateways and road leading northeastwards out of the eastern gate ! Though so plainly visible the site was not an easy one to photograph, and the only one taken is not suitable for reproduction. (It shows the features described, but not plainly enough to appear in a half-tone reproduction). The photograph has, however, been seen and passed by qualified students of Roman Britain ; nor have I the slightest doubt about the character of the site.

The implications of this discovery are far-reaching. It means that at least one other permanent Roman fort is to be looked for north-east of Cardean ; that the Romans extended their permanent occupation of Scotland at least half way up Strathmore. If we eliminate the shadowy Constantius, that advance can only have been made by Agricola or Severus. Agricolan remains have been found at Inchtuthill 11 miles SW of Cardean. A Roman road has been traced at Westmuir pointing towards Kirriemuir (it is marked on the map of Roman Britain). Other forts should be looked for somewhere in the neighbourhood of the marching-camps north of Forfar and Brechin respectively. There may be another near the marching-camp at Kirkbuddo, where a Roman road has been recorded.[4] Such a road if it existed must surely have connected a fort on the Strathmore road with a port on the coast, at Arbroath or Carnoustie perhaps.

Beyond Strathmore a line of Roman advance is indicated by the Roman marching-camps at Raedykes, Peterculter and Glenmailen. Between the last two is an alleged marching-camp near Kintore. There is also an alleged fragment of Roman road at Inverurie, but this should be regarded with a good deal of suspicion. Claims have been made for sites further north even than Glenmailen. All these should now be examined by modern methods. The route indicated is the natural route of advance to the north. Whether any permanent Roman forts will be found so far north is doubtful. The line indicated by the marching-camps reaches the sea at or near Stonehaven and then turns inland again. Somewhere here, one imagines, contact was made with the fleet, and a base established for a fresh advance. One might expect therefore to find a permanent fort at Stonehaven and two between there and Cardean.

It is perhaps significant that Cardean should be close to Meigle

[4] Maitland, *Hist. of Scotland*, 1757, I, 200. For the name and its associations see Watson, *Celtic Place-names of Scotland*, 313.

where so many sculptured stones have been found (and where they are still to be seen in the local repository). If the place was an important centre in the Dark Ages, as it clearly was, it was probably no less important earlier. Other Dark Age centres, as indicated by sculptured stones, were at St. Vigeans near Arbroath, Dunnottar near Stonehaven, and Inverurie.

It is high time, however, to curb these flights of the imagination and resume the narrative. The chief object on our return journey was to look for a Roman road up Glen Artney connecting the Roman forts near Comrie with Callander, where another fort is demanded by the topography. We failed to find any trace whatever of such a road, although much of the route consists of open moorland where it would surely be visible if it had ever existed. We had a look at Bochastle, where I had gone to look for a Roman fort in the winter, but could see no hint of anything there or in the immediate neighbourhood, though crop conditions were favourable. We turned to fly home over the Vale of Menteith and the desolate moors of Gargunnock, with a view extending from the Firth of Forth on the east to the sea and Arran in the west.

Although we had concentrated our attention upon Roman roads and sites, we had tried not to neglect entirely the remains of other periods. Native forts were so numerous that it was difficult, and sometimes impossible, to check them upon the map. We got a good bunch, as crop-sites, in the Lothians, indicating that this fertile lowland was once thickly covered with them. It is certain that if anyone could carry out a systematic air survey of this area, he would be well rewarded; but such a survey would be useless except in May or June. It should, moreover, be carried out in a series of successive years, because only when the crop grown is corn do the sites appear as a rule. (Roots, clover, grass and potatoes are not responsive to antiquity). We were also able to observe the strip-lynchets which have attracted the attention of Scottish archaeologists from the days of Gordon. I have been observing them myself for many years and I have now come to the conclusion that there are two quite distinct types: (1) the irregular scattered shelfs, such as those on the west bank of the Clyde near Elvanfoot and between Abington and Crawford; (2) the orderly series which occur in parallel rows on Shaw Hill, Culter (PLATE VII A), on the east side of Arthur's Seat, Edinburgh (PLATE VII B), and at Dunsyre, Roberton, Romanno and Old Thornielee. Those of the second class can clearly

be seen (in both our illustrations, particularly VII B),[5] both from the air and from suitable view-points on the ground, to belong to the same system as a series of riggs and furrows which abut upon them at right angles. The strip-lynchets are in fact, as I have always maintained, merely the form which the strips assume when laid out horizontally along a steep slope. I cannot conceive that any reasonable person looking at any of the sites mentioned impartially should be able to deny that both are parts of the same field-system, and were cultivated contemporaneously. Whether Type 2 developed out of Type 1 is another matter; but it well may have done so. Type 2 is plainly not earlier in date than the Middle Ages, and may have lasted down to the 17th century or later. The rigg and furrow system is exceedingly common all over the lowlands. I have recently seen good examples of it in the parish of Shotts, where it is associated with derelict farms. It is probable that some of the mysterious groups of earthworks, of which we saw an embarrassing number during our flights, consisting of rectangular and round enclosures, are in reality the remains of medieval farms and sheep-folds. They are often associated with fields of this type.

We were unable to complete the whole programme I had mapped out; but that is the normal fate of programmes. I had hoped to fly along a new Roman road into Ayrshire recently established by Mr St. Joseph, and perhaps to find some fortlets along it. I had also hoped to investigate a possible line of fortlets along the coast of Renfrew and Ayr, and to find a Roman road from Irvine to Glasgow. But these projects can well be postponed until the way has been prepared by field-work. We have at any rate profited greatly by the experience, and added a number of new sites to the map. It is evident that concentration and specialization are desirable to produce the best results; that the time and season are all important; and that air observation is most profitable when one has previously familiarized oneself with the topography by fieldwork. It is also clear that one can often classify earthworks into native, Roman and (late ?) medieval simply by looking at them from above; after all it is only thus that one can really see them as a whole at all. Thus seen, one can recognize the different types just as if they were pots or axes.

The exploration of ancient Scotland from the air has only begun. It is one of the most promising fields of research anywhere in the world.

[5] The rigg and furrow system at Culter is visible on the original photograph, but is obscured by a cloud-shadow. I observed this carefully at the time with this very point in mind, and satisfied myself about it.

Note 1

The Roman fort at Cardean is not a completely new discovery; it is rather the authentication by modern methods of a site that has long been on record. No one would have ventured to assert, on existing evidence, that it was of this character. The revelation of the rampart and road by air observation and photography now makes it possible to state with complete certainty that it is the site of a Roman fort, and further that a Roman road ran NE from the gate in its north-eastern rampart. Sir George Macdonald has kindly sent me a quotation from a holograph diary of a journey made by Roy's friend, General Melville—the discoverer of the Strathmore camps—in 1777. It is plain that General Melville saw the Cardean fort, but there is nothing in his description to suggest that he recognized it as Roman. The *New Statistical Account* (Forfar, 1845, 679) describes it as a Roman Camp which, though 'partially obliterated by agricultural operations, can still be distinctly traced'. Major Bayley, R.E., in 1861, was still 'able to identify the extent of the camp'. On 11 July 1925 I visited the site and could find no surface indications of it at all.

The Ordnance Map (Forfar 42 NE, 43 NW) records certain discoveries here, about which fuller details are entered in the original Object Name-book of 1861, compiled at the time of the first large-scale survey. The 'earthen vessels, etc., found' along the edge of the hill to the NW within the area of the fort, are described there as consisting of 'earthen vessels, human remains, iron implements and coins'; and 'other articles' were also found, at various times. The authorities for these discoveries were Mr H. J. Fitchie, of Deanfield, and David Gibb, Forester. Mr Fitchie in a letter to Major Bayley corroborated David Gibb's statement. Midway between the east gate of the fort and the farm of Wester Cardean a 'Roman sword' was found. The site of its discovery was pointed out to Major Bayley by Admiral Popham of Cardean and David Gibb; but it seems to be approximate only, as in the Name-book it is said to have been found 'hereabouts'. Major Bayley describes it as a 'straight iron sword' pronounced to be Roman by Lord Talbot de Malahide in whose possession it then was. There seems no reason to doubt this opinion; but it would be interesting to know what has become of it. Close by is the site of a 'weem' or earth-house, 'of which some of the stones indicating the site were uncovered' in Major Bayley's presence. I could find no trace of it in 1925, but did not make any local enquiries.

The Roman road, too, has quite a respectable pedigree. The Ordnance Maps mark it only at Westmuir, 6 miles NE of Cardean and 1½ miles SW of Kirriemuir. Maitland (*Hist. of Scotland*, 1757, I, 200) refers to it at Westmuir ('Riddy').[6] The *New Stat. Account* says definitely that there was a Roman road running east from the camp at Cardean along Strathmore. In 1861 the Ordnance surveyor described the fragment at Reedie (Westmuir) as 'a small portion of an old road raised about 1½ feet above the level of the adjacent ground'. Major Bayley says that the 'whole of the distance here mentioned' (*i.e.* in the *N.S.A.*) has been more or less identified; but it is not clear whether he is referring to the whole course from Cardean to Westmuir or merely to the 500 yards then visible at Westmuir; probably the latter, for otherwise he would surely have inserted its course on the Ordnance map.

I wish here most gratefully to acknowledge the courtesy of the Glasgow Archaeological Society in allowing this preliminary notice to precede its own much more substantial publication of the results, and to thank Mr St. Joseph for his valuable cooperation. For the statements in this article, however, I alone must be held responsible.

[6] Roy investigated it in 1771 (M.A. 108).

Pliska*

by Iwan Welkow

Director of the National Museum, Sofia

WEST of the Provadia hills in Bulgaria is the valley of Shumen. The northern part of this valley belongs to the basin of the Provadia river, the southern to that of the river Kamtchia. The present railway line from Shumen to Kaspitchan serves as an ideal boundary between these two regions, but the two valleys are best known because of two historical sites, Preslav and Aboba (now Pliska).

The rivers of the Aboba plain have very little water during the summer season, but during winter and in time of heavy rains they overflow and cover great parts of the barren valley. The winter there is cold and severe, the summer dry and hot. In the plain, north of Kaspitchan railway station, are the ruins of Aboba. Eshberüch or Isperich (Asparuch), the younger son of Kurt (644–702), in his march southwards stopped there, and founded the first capital of the Bulgarian khans, later to become the magnificent Pliska.

For centuries the ruins of Pliska were almost completely forgotten. None of the medieval travellers mentions them, nor do any of our chroniclers. The roads changed their course southwards and in time even the name of the old fortress was forgotten. Early in the 19th century the name of Aboba first begins to be mentioned again, when its ruins attracted the attention of travellers. Niebuhr, in his *Reisebeschreibung nach Arabien* (Hamburg, 1837) spoke of traces of an old settlement in the vicinity of Aboba and Novi-Pasar. Later Kanitz drew attention to the ruins of Aboba. In his book *Donau-Bulgarien und der Balkan* (1882, vol. III, 254) he wrote: 'Following the directions which were given to us, half an hour from Aboba I found a large plain full of ruins with the towers and fortress-walls of a well-preserved Roman castle, of whose existence there was no previous record'. Basing his observations on the inscriptions found there, he assumed it

* Translated from the Bulgarian by Dr Ivanka Akrabova. Published by 'Bulgarian Starina', Shumen, and printed at the Court Printing Press, Sofia, 1930.

to be Burdisus, but immediately afterwards threw doubt on his assumption, because Burdisus Bulgarophigon, the present city of Baba-Eski, is to be found between Adrianople and Constantinople. He also says ' if so large and so well preserved a castle were ever to be discovered in our country, it would draw general attention, and many archaeological and historical monographs would soon be written about it '.

Ireček passed through the same places a few years after Kanitz. He had better opportunities of observing the ruins well. Ireček too, at first thought that they were Roman, but identified them with Abrittus in Misia, which, as Škorpil proved, is to be located near Abtaat in the Dobrudja. Ireček also thought they might be Pliska, mentioned by the Byzantine writers Leo Diaconus and Anna Comnena.

Karl Škorpil was the first to point out that the earliest capital of the Bulgarian khans should be looked for near the present Aboba. Although he did not bring forward much evidence in support of his opinion, it seemed so probable that it did not meet with any opposition. Even Ireček declared later that the old Bulgarian capital must not be looked for near Preslav, because its geographical situation is not suitable for a cavalry camp, but rather at Pliska. There, according to him, was the main camp, or ' Hring ', of the proto-Bulgarians, whose horses, according to the testimony of Masudi, grazed freely over the meadows, ready for a sudden march.

The excavations of the Russian Archaeological Institute began at Pliska in October, 1889. The first object was to excavate the central mound where, according to the local peasants, a great fortress once existed. In 1900 the excavations were resumed in the vicinity.

The camp of the proto-Bulgarians had been there even in the eighth century, when the earthworks were thrown up. For in the plain of Aboba there are two kinds of defensive constructions, of earth and of stone. Some of the earthworks are very well preserved, but the majority have disappeared completely, especially in cultivated regions. There are earthworks also in Novi-Pasar, but they are of Turkish date.

The Great Earthen Rampart forms an irregular quadrangle ; the north side is 3.9 km. long ; the west and the east sides are nearly the same (7 km.) length ; the southern side is 2.7 km. At present it is impossible to determine the exact number of original entrances. Only where remains of made roads can still be found can we presume that there have been entrances of some kind. There were two roads going northwards, one eastwards and one westwards. In the southern part there was an entrance near Assar Dere. (FIG. 2, p. 297).

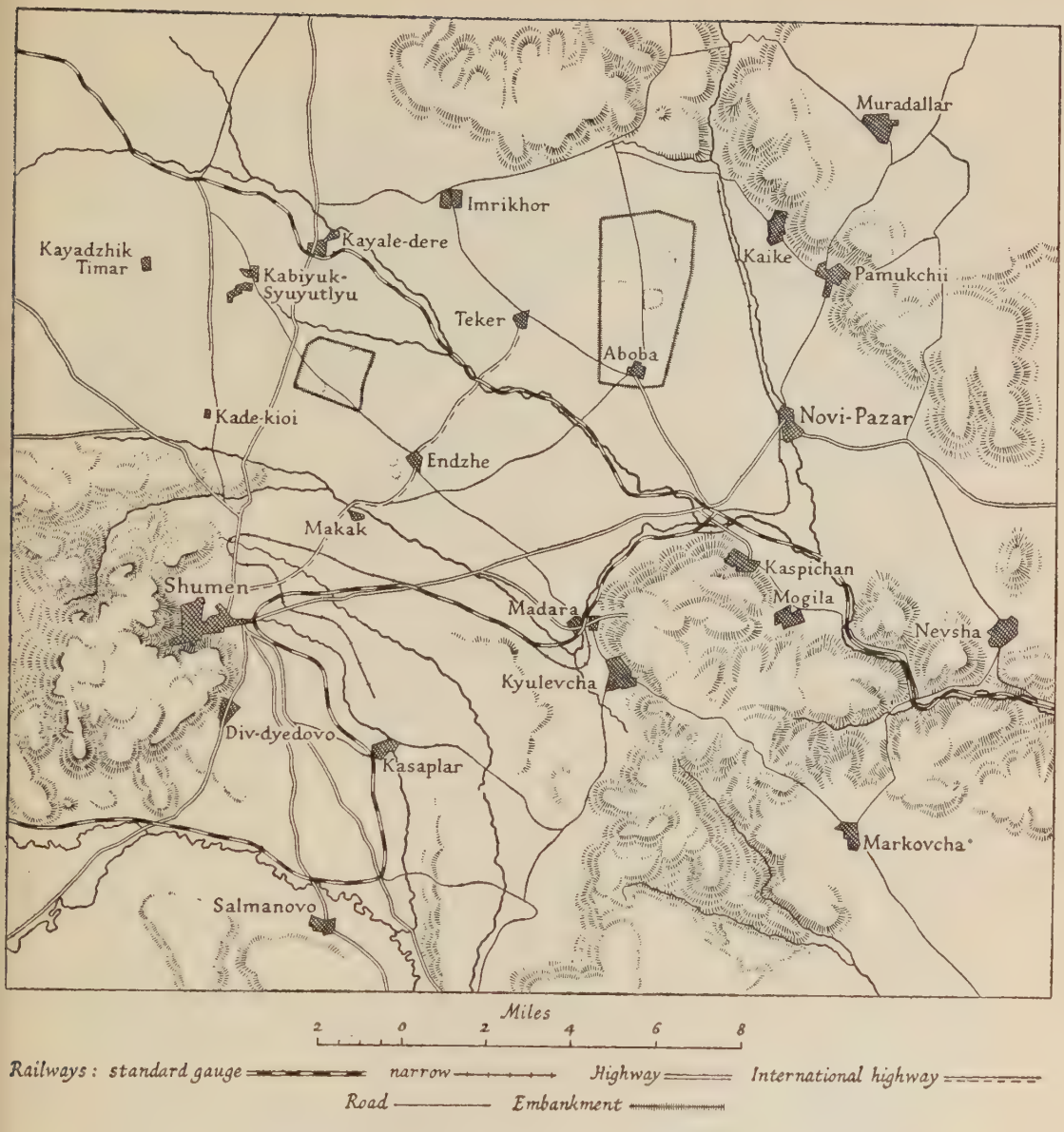


FIG. 1. THE VALLEY OF SHUMEN AND THE SITE OF ABOBA (NOW PLISKA), BULGARIA

The bank of the rampart is on the inner side. Its measurements vary according to the terrain. It is 12 m. wide at the bottom and near the surface, 3.5 m.

In the centre of these huge earthworks, which enclose an area of no less than 23 square kilometres (9 square miles), is a stone fort, built at the beginning of the ninth century. In the inscription of Omortag, as also in an apocrypha speaking of the vision of the prophet Isaiah and in the Byzantine writers, this stone fortress is mentioned and called Pliska. It is known that Bolgar, the capital of the Bulgarians settled along the river Volga, was also surrounded by earthworks; there still exists a linear earthwork, 400 km. long, extending from the Volga to the river Kama. This type of construction the Bulgarians learned from the Huns, who in their turn were influenced by the Chinese.

The ruins of the Pliska fortress have the form of a trapeze with the western and the eastern walls parallel. The western wall is 788 m. long, the eastern, 612 m. The southern and the northern are almost equal, 740 m. each. The walls are 2.6 m. thick, and are built of large, ashlar blocks, evenly laid.

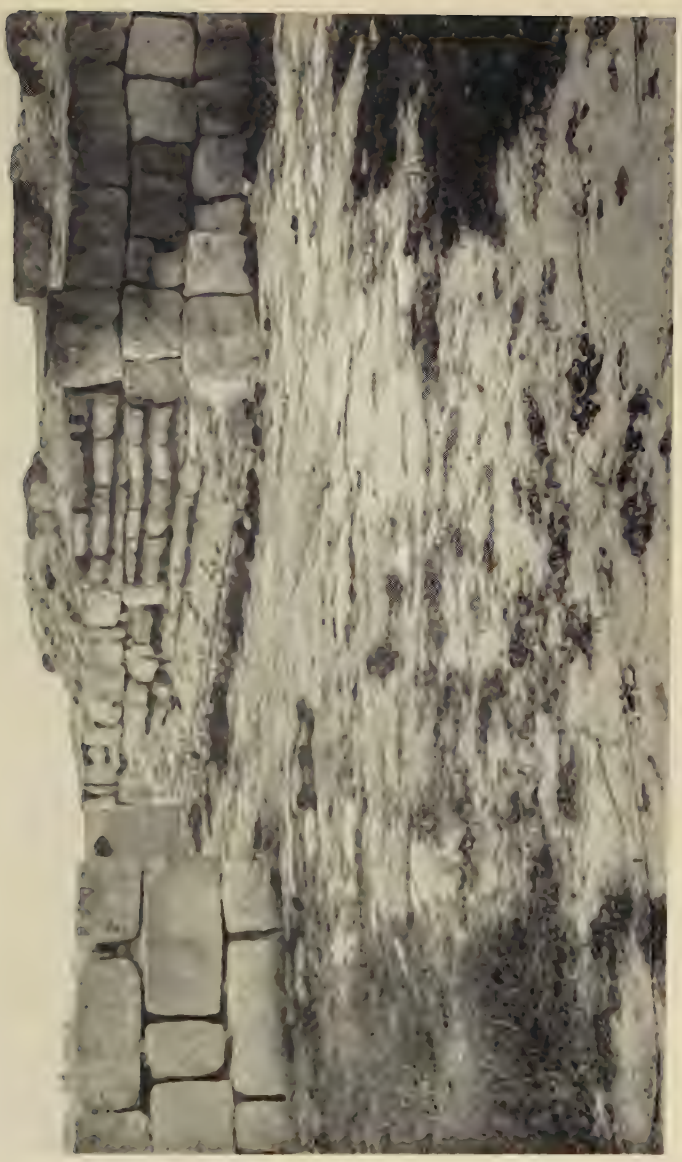
There was a round bastion at each corner of the fortress, with a pentagonal one on each side. The fortress had four gates, flanked by square double bastions. The main entrance was the eastern. It is assumed that the walls were 10–12 m. high, the only access to them being by way of the bastions. The eastern gate was 4.7 m. wide and probably vaulted with bricks. There were two gates, one of which could be lowered (PLATES I, II).

The remains of great stone buildings, found in the inner fortress, show that here we are dealing with the ruins of a large city. All these constructions seem to date from the time of Omortag. This postulated date acquires some probability from the known fact that in 811 the residence of the Khan Krum at Pliska was surrounded and burnt by Nicephor. It must, however, be admitted that the stone buildings in the fortress date from the period after the fire. The ruins of this fortress called Kale-eri, are also known by the name of Sarai-eri, *e.g.* place of the palace.

The largest and the most important of the ruins are those of the Great Palace or Throne-room. This was a monumental construction. It had the form of a rectangle 52 by 26.5 m.; the outer walls are 2.2 m. to 2.6 m. thick; but the southern wall is less, 1.6 m. (PLATE III).

A transverse wall (maximum thickness 1.6 m.) divides the Throne-room into two parts. The northern part is a hall 31.4 m. long,

PLATE I



EAST GATE, LOOKING WEST (*see* p. [illegible])
Ph. O. G. S. Crawford

facing p. 296

PLISKA: ROUND TOWER AT SOUTHEAST CORNER OF INNER FORT (*see* p. 296)
Ph. O. G. S. Crawford

PLISKA: GENERAL VIEW OF THRONE-ROOM, INNER FORT, LOOKING NORTHWEST (*see* p. 296)

Ph. O. G. S. Crawford

PLATE IV

INSCRIPTION BUILT INTO WALI OF PALACE AT PLISKA, NEAR SHUMEN, BULGARIA (*see* p. 301)
Ph. O. G. S. Crawford

consisting of four corridors—flanking corridors are each 3 m. wide, the central west, 4.6 m., the central east, 5.8 m. These corridors were connected by wide entrances, maximum width 2 m., so that it was very

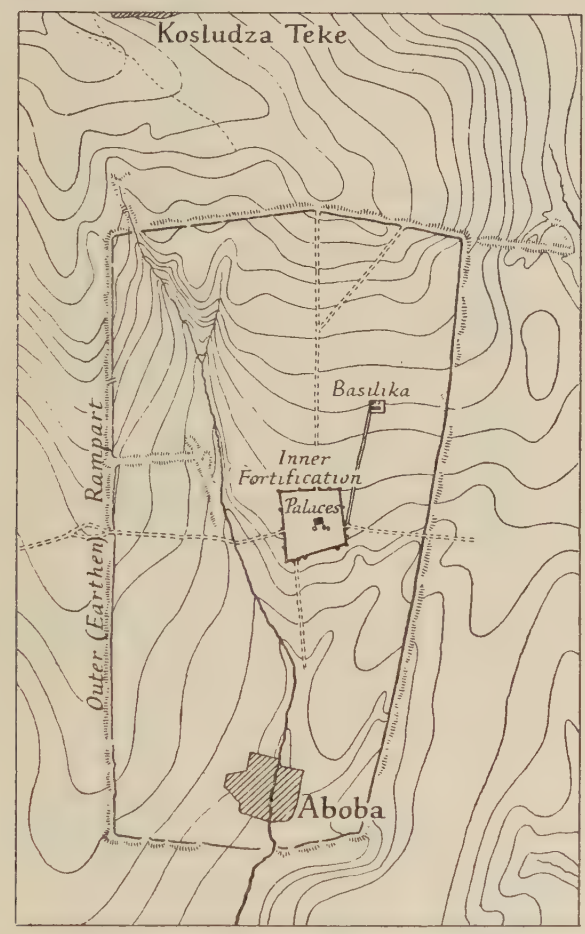


FIG. 2. FORTRESS AT ABOBA (PLISKA), BULGARIA

easy to pass from one end of the throne-room to the other. Most probably the throne stood at the north apsidal end. It is accessible to all the different halls (FIG. 3).

The architecture and solid proportions of the hall have a purely oriental character, very different from that of Byzantine buildings. The

use of large, square stone blocks was not known in these parts before the coming of the Bulgarians. Traces of similar construction are seen in the oldest parts of the fortifications at Preslav and Tirnovo. The origin of this type of building is found in Sassanian architectural traditions, whose monuments consist of palaces built with similar stone blocks. Even in plan these palaces are completely similar. This is easily explained by the fact that the Bulgarians, before their westward migration, lived for centuries in close contact with the Sassanide Empire.

The Small Palace adjoins the northern wall of the enclosure which surrounds the court and church. Rectangular in construction, its measurements are 23 by 19 m. The western wall is 1.6 m. thick, the others about 0.95 m. The interior of the palace is divided into rectangular divisions and subdivisions. and the narrow corridor-like enclosures are particularly interesting. The floor of the northern enclosure is covered with 12 rows of stone slabs, 6 in each row. The western corridor at its beginning is divided into three parts. In the southern one there are preserved remains of a staircase which led to the second floor. There are two kinds of walls in the Small Palace : the first is built with regularly cut stone blocks and mortar ; the second consists both of artificially cut blocks and unhewn blocks. The roof must have been of wood. It is assumed that there was a second floor, but it is difficult to tell how it was built. In the adjoining yard there have been found remains of wooden constructions, and the bones of domestic and wild animals. Here most probably were the kitchen quarters. (FIG. 4).

West of the Throne-room is the Palace Chapel, the remains of which are of pre-Christian and Christian periods. The older construction is contemporary with the palaces and measures 24 by 15 m., with a long wall eastwards (FIG. 5). There are no interior walls. It is assumed that this building was a pagan shrine of the early Bulgarians. After the acceptance of Christianity in Bulgaria the old Pagan temples were usually converted into Christian churches and this process was repeated in the Balkan peninsula after the Turkish invasion, when the Christian churches in their turn were converted into Mosques.

In order to reach the large Christian basilica outside the fortress one must pass along the old paved road, the only one preserved. This road proceeds from the Throne-room, passes through the main eastern gate of the fortress and then turns northwards. It is 2.5 m. wide and is paved with large stone slabs, some of which are 1.8 m. long. The basilica was built in an open space, about 1 km. northeast of the

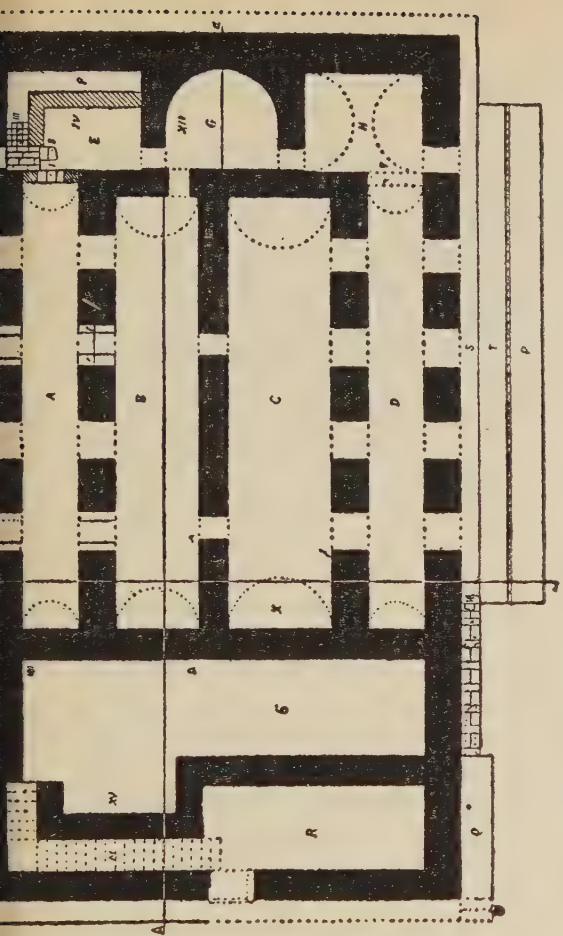


FIG. 3. THE GREAT PALACE, PLISKA

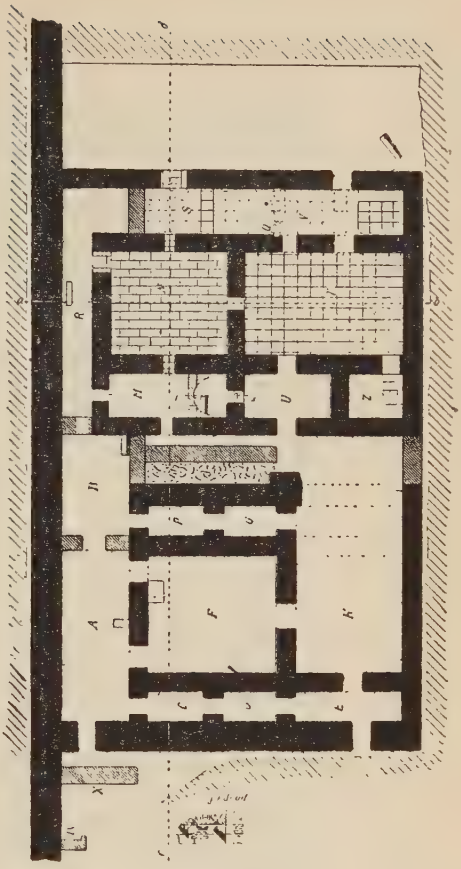


FIG. 4. THE SMALL PALACE, PLISKA

FIG. 5. PALACE CHAPEL, PLISKA

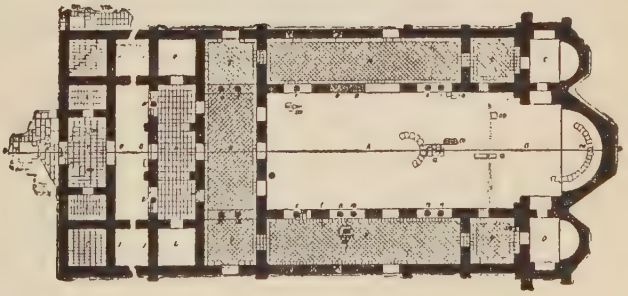


FIG. 6. BASILICA, PLISKA

fortresss but within the great earthen rampart. The site consists of two terraces, the church proper lying on the north terrace, with a southern terrace serving as a court. The church was completely isolated and only the road connected it with the fortress. It was surrounded by a square enclosure not strong enough for defence.

The church, before it was excavated was known locally by the name of Klisse-eri. There is a legend that all the treasures of the church and the royal palace were thrown into a well. According to the same legend this well lay somewhere to the southwest of the southern entrance of the church. A well has been discovered here, but it is northeast of the church.

The basilica of Aboba shows distinct traces of eastern influences. It has the form of a rectangle with three apses, and is 100 m. long and nearly 30 m. wide (FIG. 6).

The main entrance was on the west side. There is a narthex and atrium, both 50 m. long—nearly half the whole length of the building. At the corners there are square constructions which must have been towers. The side aisles are separated from the nave by heavy pillars, between which were marble columns. In the ruins have been found several marble capitals and column bases. They vary in form and ornament, suggesting that they were not made specially for the church but taken from other buildings. The custom of bringing away marble columns and other monuments as booty after a battle was very popular with the Bulgarians. In 814 Khan Krum reached the walls of Constantinople, and on his way there plundered many churches and monasteries. When he returned to Bulgaria he brought away columns and other monuments of Greek art. According to Ireček such columns were placed in front of the palaces of Khan Krum and were used later in the building of the new temples.

In the middle of the nave have been discovered remains of a pulpit, and in the southern aisle a well, most probably connected with a baptistery.

In many places and particularly in the ruins of the basilica have been found columns with short inscriptions. These usually have a cross at the beginning, followed by the name of a town-fortress or, less frequently, the mention of a war. In the basilica of Aboba were found columns with the names of Rodosto, Burdisus, Dimotika, Visa, Arkadiopolis (Liule Burgas), Sosopolis, Mesembria, Scutarion, Seres. Judging from those found in the narthex of the nave, there were originally sixteen such columns. Ireček thinks that they were placed

in front of the palace to proclaim the number of cities conquered by the Khan ; but Uspensky, who directed the excavations, thinks that they were installed in the big church, built by Simeon near the royal palaces about 924, which is also the approximate date of the basilica. Prof. Zlatarski accepts Ireček's conclusion.

The basilica of Pliska is one of the largest of the old buildings found in Bulgaria. We know that during the reign of the first Bulgarian Christian ruler, Boris (852–889), many cathedrals were built. The material of all the public buildings comes from the country around Aboba, Shumen and Devnia. The nearest and perhaps the main source available was the ruins of the Roman city of Marcianopolis. The records of this city are interrupted about the date of the foundation of Pliska.

Among the old Bulgarian inscriptions written in Greek, derived originally from Pliska and its neighbourhood and later removed elsewhere, the most important are the column inscriptions composed in honour of certain men for their services to the government and the people. (PLATE IV). Similar monuments were erected not only after the death of these men, but also during their life-time. On all these monuments, with very few exceptions, occurs the name of Omurtag or Malamir. They are the only documents we possess relating to old Bulgarian social customs, which were extinguished by the introduction of Christianity and the ensuing political changes. These inscribed columns are very important from the point of view of philology and history, and that not only for the Bulgarians, but also for the other eastern peoples of the migration-period in general.

The inscriptions do not vary much in their general content. They generally state the title of the Khan, or other person who ordered them to be written, the name of the person in honour of whom the column was erected, and his position in the goverment, personal relations with the Khan, merits, origin, and kin.

Thus we read : ' Khan Übigi Omurtag. Ohshuan, the Jupan* Tarkan,* was my adopted man and died in battle. His kin was Kurigir.'

Of particular archaeological importance is the lost inscription of Kavhan Ishbul : ' Khan Jubigi Malamir, prince from God. His old Boila* Ishbul built the aqueduct and gave it as a present to the prince ; and the prince gave to the Bulgarians to eat and to drink many times and to the Boils* and the Bagains* gave many presents. May God allow

*These untranslatable words are titles of nobility.

the prince to live a hundred years together with Kavhan Ishbul '. The Bulgarian Khan Malamir (831–836), son of Omurtag, erected a column in honour of the Velmodza, the Kavhan Ishbul, who built an aqueduct, most probably in Pliska. In the inscription are mentioned the festivals held in honour of the opening of the aqueduct. These were the famous festivals of the proto-Bulgarians held on different occasions, as for instance at the conclusion of an important government undertaking. But the most imposing were those held to commemorate an important personality.

Pliska is the probable source of other important historical inscriptions. Of these we must mention first the column of Suleiman Köi, which was taken there from Pliska. This monument must belong to the time of Omurtag and contains the important treaty of 814, between Bulgaria and Byzantium. There is a fragment from the basilica with the date 1142 on it (in the reign of John Comnenus); on another fragment is the monogram of the Paleologs, and on another the Slav inscription Делань Бань, which is the name of a well known person of the eleventh century. The inscriptions show that these regions did not lose their importance with the coming of Christianity, and that they were not completely forgotten or deserted in the eleventh and twelfth centuries.

In all the buildings are found stones, bricks, tiles, and aqueduct pipes with different signs on them. They are probably not writing, but a magical means to catch and concentrate thoughts on a particular object. From all that is known of the old culture of the proto-Bulgarians it is natural to assume that they had some kind of writing, which consisted of dots and dashes, as mentioned by Černorizetz Khrabr. The problem of the existence of a Slav alphabet before the adoption of the Cyrillic is not new. These mysterious signs still await solution.

The numismatic material from Pliska is not very rich. It consists of a few lead seals and several dozens of Byzantine coins. The most important of the lead seals is the one of Vasili II (976–1025). On the obverse is represented the Emperor. In his right hand he holds the cross and orb, in the other a small sack of earth—the acacia, symbol of the man's transitory existence. Lastly there was found at Pliska a lead seal with a representation on the obverse only and the inscription : Peter, king of the Bulgarians.

In the neighbourhood of Pliska there are several other interesting monuments. Such are the so-called Devtashlari. These are colossal stones standing in regular or irregular groups. In front of these groups is a stone shaped like a throne and facing the group. Excavations

around the Devtashlari have not yet yielded any definite results, nor have they disclosed the epoch to which they belong. On some of them the proto-Bulgarian sign-writing has been found. They may have served for some ceremonies of which we still know nothing. The stone statues, called Babi, discovered near Endze (in the neighbourhood of Shumen) will bring us closer to the solution of this problem.

It is supposed that Isperich founded the capital of the Khans. Pliska was first attacked by the enemy in 811. Later we find there the residence of Omurtag, who was the chief builder of the forts and palaces.

It is recorded that in 885 there were in Pliska the three disciples of Methodi : Climent, Naum and Angelari ; they were banished from Moravia and sent to King Boris.

Pliska must already have lost its importance as a capital in the time of Boris. This fact is attested after 893 when Simeon was chosen king by a national assembly. These regions must have been invaded by the Magyars in 895, and by Ivan (Zimiscès) in 972, when he took Pliska on his way to Drester.

A second invasion is recorded in 1001 when the leaders of the army of Basil II, after taking Veliki Preslav and Malki Preslav, also took Pliska. Worst of all was the invasion of the Petchenegs in the second half of the eleventh century. Everything was plundered, and the monuments of the Bulgarians were destroyed or carried away. Later the capital of the Bulgarian state was moved elsewhere. Pliska and Preslav, plundered and destroyed, lost their political and cultural importance for ever.

Ras Shamra, 1929-39*

by T. H. Gaster

IT is now ten years since the French archaeological expedition under M. Claude Schaeffer first started digging at Ras Shamra, on the north coast of Syria. The present is therefore a suitable moment in which to take stock of what has so far been discovered.

Our best point of departure is provided by the series of annual volumes in which the excavator's reports and the epigraphists' primary studies have been conveniently gathered together from the pages of the periodical Syria. It must be borne in mind, however, that these excellent publications represent a *preliminary* and not a *final* stage in the interpretation of the materials. The literature grows from day to day, and the contributions made by other scholars are equally important for a correct appreciation of the subject. Among these special mention should be made of the work of Albright, Montgomery, Harris, Goetze and Gordon in America, of Ginsberg and de Vaux in Palestine, of Friedrich and Eissfeldt in Germany, of Hrozný in Prague, Aistleitner in Budapest and Cassuto in Italy. In this country studies have been published by the late Father Burrows, Dr J. W. Jack, of Edinburgh, Professor S. H. Hooke and the present writer.

Stratification of the Site and History of Ugarit

It is now established, on the basis of texts found there, that the mound of Ras Shamra conceals the ancient city of Ugarit, long known to students both from the Amarna Letters and from sundry references in Egyptian and Hittite records.

Ugarit possessed one of the few natural harbours on the Syrian coast, and it is but a day's sail from the important island of Cyprus, the

* Claude F. A. Schaeffer and Ch. Virolleaud. Les Fouilles de Minet el Beida et de Ras Shamra (première campagne), 1929. The second to the ninth reports were published in Syria, 1931–8 (Geuthner, 12 rue Vavin, Paris).

Claude F. A. Schaeffer. The Cuneiform texts of Ras Shamra (Ugarit). The Schweich lectures of the British Academy, 1936. Oxford University Press, 1939. pp. xvi, 100, 39 plates. 8/6.

jutting promontory of which lies almost directly opposite. It was therefore destined to serve as a trading-centre between East and West. Merchants from all parts poured into it, and it served as the terminus for caravan-routes leading variously to Palestine in the south, Mesopotamia and the Hurro-Mitannian territories in the east, and Asia Minor in the north. It is this constant crossing and blending of cultures which created the peculiarly syncretistic civilization of the city during its heyday, and it is this which gives it today its primary interest. Ugarit must be regarded as a kind of Cairo of the ancient world.

Archaeological evidence points to occupation of the city from neolithic times down to the twelfth century B.C., but the exact course of its history cannot yet be determined. Details of the stratification, tabulated from M. Schaeffer's excellent reports, are given on the accompanying table. It must be observed, however, that the latter does not profess to set out every detail, but merely to present the more essential features of each level.

From the table (see page 306) it will be seen at once that the heyday of the city, and the period concerning which information is most abundant, falls during the epochs of the Fourth and Fifth Settlements, extending approximately from the twenty-first to the twelfth centuries B.C. and corresponding to the age of the Twelfth and Eighteenth Dynasties of Egypt.

The preceding periods (First-Third Settlement) are extremely obscure. Certain cultural contacts are perhaps suggested by the affinities of the pottery with those found at other North Syrian sites, but such evidence must necessarily be treated with reserve. We know far too little about the origin and distribution of the types concerned to justify the conclusion that their occurrence at any two sites proves an immediate relationship between them. There were probably several intermediate stations and future excavation can alone settle the question.

An interesting feature of Second Settlement pottery is the tendency to use in decoration the *motif* of a row of human eyes. This recalls the employment of a similar motif in the early architecture of Brak and, if it be really apotropaic in character, as Schaeffer suggests, it provides important evidence for the antiquity in Syria of the belief in the Evil Eye.

From the period of the Second Settlement onwards, the influence of Egypt is especially dominant, due, no doubt, to the results of the policy of imperialist expansion pursued by the Pharaohs at this period. The copper mines of the Lebanon area, and the plentiful supply of cedar-wood available in the district would naturally encourage such ambitions,

RAS SHAMRA—UGARIT—STRATIFICATION

Level	Particulars	Date
I (a)	Cosmopolitan culture; invasion of 'sea-peoples', (cf. Malalas' story that Kasos, *i.e.* eponym of Casiotis region, married Kitia, eponym of Kition).	XIIth cent. B.C.
I (b)	From XIVth cent. Mycenaean ware is prominent. Rectangular tombs with vaulted ceiling and small *dromos*. Hurrian influence abounds.	XIV–XIIIth cent. B.C.
	Conflagration	
I (c)	From XVth cent. onwards, tombs with exclusively Cypriotic ware. *Fifth Settlement*	XV–XIVth cent. B.C.
II	Distinguished by profusion of 'Phoenician' ware. Two Semitic temples, one dedicated to Ba'al and the other to Dagan. Bronze ware from Cyprus. Level dated absolutely by coraline pearl bearing name of Sesostris I, and by statue of the wife of Sesostris II (1906–1887 B.C.). *Fourth Settlement*	XXI–XVIth cent. B.C.
III (a)	Painted ware—dark brown, violet or black on buff background—thin—unparalleled on Syrian coast, but possesses counterparts at Tepe Giyan and Susa I *bis*, as also in ware found by Ingholt at Hama. Designs sometimes incised on clay whilst still wet. Polished axes. Obsidian blades.	Third Millennium
III (b)	Unpainted pottery with red glaze. Also, distinctive polychrome pottery, red or black on buff clay. Jar-burial. *Third Settlement*	First half of Fourth Millennium B.C.
IV	Painted pottery: triangles—zigzags—vandykes—meanders—dots—frets—rows of human eyes. Vessels are globular with flat bases—horizontal handles—black lustre on dull clay—incised and scratched designs. Metal unknown. Blades of silex. Small axes in hard polished stone. Bone bodkins and awls. In earlier level: thick dull clay ware, with yellow or bright red glaze. Reminiscent of so-called 'neolithic' ware from Cyprus (ANTIQUITY, 1934, VIII, p. 86). Earlier period has parallels at Sakje Geüzi, Khan Yunus, Samarra, Susa I and Tepe Mussian. *Second Settlement*	First half of Fourth Millennium B.C.
V	Unpainted ware—ledge handles—implements in flint (silex) and obsidian. Primitive ornamentation of vessels by means of scratched vertical lines, irregularly parallel, or simple incised patterns. *First Settlement*	Neolithic

whilst the control of so important a harbour would be of distinct commercial advantage.

Positive evidence of this influence during the period of the XIIth Dynasty (Fourth Settlement) is afforded by the discovery, south of the great temple, of a broken statuette of Chnumit, bride of Senusret III (1906–1887 B.C.), whose tomb has been unearthed at Dashur. To the same period belong two sphinxes inscribed with the name of Amonemhet III, and a group featuring three persons, one of whom is the XIIth Dynasty official Senusret-onkh.

That Ugarit was also in contact at this period with other important states is proved by a letter, recently discovered at Mari (Tell Hariri) and dating about 2000 B.C., in which a certain Hammurapi, king of Kurda (*not* the great monarch of Babylon) informs Zimrilim, king of Mari, of a desire expressed by the governor of Ugarit to view the beauties of the latter's palace (Dossin, SYRIA, XIX, 125 f).

In the time of the XVIIIth Dynasty, our information concerning the history of Ugarit is eked out also by external records, most important of which are the celebrated Amarna Letters. One of these, addressed by Abimilki of Tyre to Akhenaten informs him that half of the city of Ugarit has been consumed by fire (Winckler, 151, 55). M. Schaeffer has ingeniously suggested that this fire may be identical with the conflagration the traces of which separate the second from the third layer in Level I. He has furthermore pointed out that in the ruins of a house dating just before the conflagration was found a scarab inscribed with the name of Amenhotep III who died in 1376 B.C.

Later, in the XIXth Dynasty, the name of Ugarit turns up again among the allies of Rameses II (1301–1234 B.C.) in his battle against the Hittites at Kadesh (1283 B.C.). After that, it disappears from the pages of Oriental history, except for sporadic and unimportant references in the Hittite documents from Boghaz Kewi. The prevailing view is that it fell a victim to the Syrian campaigns of Tiglath-Pileser I (*c.* 1100 B.C.). Before this occurred, however, the Hurro-Mitannians had already gained supremacy. The only two kings whose names we know, viz. Niqmedaš and Astari, bear Hurrian names, and in the ritual texts discovered on the site (v. *infra*) Hurrian deities are mentioned side by side with those of the Semitic pantheon. Moreover, at least 40 per cent of the proper-names occurring in those documents are Hurrian and, as will be described presently, texts written in a language closely resembling, if not identical with, Hurro-Mitannian turn up among the written archives of the city.

Ceramic evidence shows that early in the period of the Fifth Settlement the influence of Cyprus began to make itself prominent. Similarities with the so-called ' neolithic ' ware of this island occur already in the time of the Second Settlement (IVth Level from the top), but it is only at this late period that the influence becomes really marked, and it is then coupled also with evidence of Mycenaean contacts. It is probable, therefore, that Ugarit, like many another coastal city, felt the pressure of the Sea-peoples, but the exact extent of their domination cannot yet be determined. The present writer is inclined to suggest, however, that ethnic conditions such as must have obtained at Ugarit during the later centuries of the First Settlement are portrayed by the North Syrian seer Balaam in the celebrated passage, Numbers XXIV, 24. This passage, the antiquity of which is, despite its later redaction, surely to be recognized, runs as follows in the Revised Version :

> But ships shall come from the coast of Kittim (Kition, in Cyprus), and they shall afflict Asshur (Assyria) and shall afflict Eber (the Hebrews).

It is, however, quite absurd to speak of Cypriot ships as oppressing far-distant Assyria. Therefore, the writer would suggest that Asshur and Eber are *subjects*, not *objects*, of the verb ' oppress '. The sense will than be :

> There shall come ships from Kition, and both Assyria and the Ḫabiru shall bring about oppression.

This corresponds exactly to the situation in North Syria during the period in question. On the one hand was the invasion of the Sea-peoples and of the marauding Hurrian and Semitic Hebrews, and on the other the later conquest by the Assyrian forces of Tiglath-Pileser I. If the passage were written at any time after 1100 B.C., it would reflect most faithfully the troubled state of North Syria at this era of its history, and might thus be taken to portray the conditions in which Ugarit then found itself.

This is as far as we can reasonably go today in sketching the history of Ugarit.

The Texts

More important than anything else discovered at Ras Shamra are the Cuneiform Tablets. These all come from the First Level, and from a layer below the remains of the great conflagration. They are therefore in any case older than 1375 B.C. Some of them, containing mythological poems, were found in the precincts of the temple-library

and may be dated approximately about 1500 B.C., though their actual contents, being traditional, are no doubt older still. Others, unearthed at different parts of the site, are not of uniform date. Some may be earlier than the sixteenth century, some later. It is impossible to tell. All, however, belong to the same general epoch of the Fifth Settlement.

The texts divide readily into two main classes, viz. (a) those written in the usual *syllabic* cuneiform, and (b) those written in an hitherto unknown cuneiform *alphabet*. The former are written both in Accadian and in a language, as yet imperfectly understood, akin to Hurro-Mitannian. The language of the latter may best be described as proto-Hebrew. It is an earlier stage of the language in which the Old Testament is composed, and consists of a blending of several Semitic dialects, some North-Western, or Amoritic, and some Southern, or Arabic.

The origins of the cuneiform alphabet are as yet unknown. There are two principal theories. The one, propounded by the German Assyriologist Ebeling, asserts that the signs are in reality nothing but curtailments of normal syllabic characters. By omitting a wedge here and a wedge there signs which, say, originally stood for *ma* or *pa* were made to do duty for the corresponding alphabetic values *m* and *p*. The other theory, sponsored by Olmstead, the late Father Burrows and the present writer, asserts that the Ras Shamra script is an adaptation to the exigencies of wedge-writing of that ancient Semitic alphabet, parent alike of the later Phoenician and Greek systems, traces of which have been recovered in recent years both at Serabit al Khadim in Sinai and at various Late Bronze Age sites in Palestine, such as Tell Duweir, Beth Shemesh, Gezer and Shechem. It must be confessed, however, that as yet neither school of thought has been able to produce a connected epigraphic chain in support of its thesis, and the problem becomes increasingly complicated by the succession of new scripts which are constantly turning up in Syro-Palestinian sites, as for instance at Byblus.

One objection which has been raised against the second theory, and which in some quarters has been regarded as decisive, is the fact that the Ras Shamra script possesses three distinct signs for the letter *aleph*, depending respectively on whether it is vocalized as *a*, *i* or *u*, whereas the palaeo-Hebraic alphabet has but one sign. This objection is, however, by no means final, for the fact is that two of the Ras Shamra *aleph*-signs are but secondary formations from the letter *h*, and therefore of later introduction. In the original cuneiform alphabet, therefore, there was only one *aleph*-sign, as in the palaeo-Hebraic, and it is to

the original form of the script, not to its development at Ras Shamra, that we must turn when we seek to determine the question of origin.

Be this as it may, and whatever the ultimate origin of the script may have been, it is noteworthy that another specimen of it, though written retrograde, has been found in the South Palestinian site of Beth Shemesh. This at least shows that it was not exclusive to Ugarit. Moreover, the very fact that it is there written retrograde, as also on a few Ras Shamra documents, might point to an origin in some system of writing, like that of Serabit al Khadim, wherein the letters could, in fact, face in either direction.

The contents of the Semitic tablets are extremely varied. Several of them, discovered during the first campaign of 1929, are ritual tariffs, defining sacrifices to be offered in the temple. They are especially interesting because many of the sacrifices bear names long familiar from the codes of the Pentateuch showing that, however late be the literary redaction of that work, the material incorporated is genuinely ancient and belongs to that early Hebrew cultus from which the Ugaritic and the Israelitic (as well as the Phoenician and later South Arabian) systems were alike evolved.

Another interesting feature of these texts is that in some cases a line is drawn across the tablet after the main body of their contents, and thereafter is specified expressly the part to be played in the ritual by the king. This shows that, as in Mesopotamian cultus, the king played a distinctive role in temple-ceremonies, and it further recalls the later Israelitic distinction between the sacrifices offered by the king and those offered by the people (cf. II Kings, XVI, 15). In one document (IX, 10–11) we actually hear of the king's performing a ritual act of purification (*m-l-k b-r-r*) in the new-year month of Teshrit (*y-r-ḫ T-ś-r-t*). This perhaps suggests a ritual period of purification preparatory to an annual renewal of kingship at the season of the New Year—a custom which is not without parallels.

Another text (no. III) appears to prescribe sacrifices for a seven-day festival, and this precedes the day of new moon (*y-m ḥ-d-š*). It is therefore probable that the ritual in question was designed for the great week of celebration which marked the beginning of the year, for according to the recent conclusions of Professor Julian Morgenstern the ancient Hebrews observed this occasion just before the new moon.

We also read in these ritual texts of offerings on the roof-tops, such as were denounced by the Israelitic prophets, and likewise of the erection to the deities of cathedrae, or little seats (*m-š-b-t*), recalling the ' seat '

(*mōšab*) of the Idol of Jealousy seen by the prophet Ezekiel in the forecourt of the Temple at Jerusalem, and denounced by him as a heathen practice (Ezekiel, VIII, 3).

A particularly interesting document (no. XVII) gives a list of deities worshipped at Ugarit, and in this list we find such Semitic gods and goddesses as Astarte, Resheph, ' the god of the North ' (*E-l Ṣ-p-n*) and Shalem (whose name survives in Jeru-salem) honoured side by side with members of the Hurrian pantheon like Ishkhara and the obscure D-d-m-s, reflecting the mixed population of the city at this period.

Besides the ritual texts, there are also other minor documents of great interest. One of these is a treatise on the cure of equine disorders, among which is mentioned the bronchial condition still known as ' roaring '. Herbs prescribed in remedies include sesame, juniper, licorice, and mint. A poultice of figs is also mentioned.

Another tablet contains a schoolboy's exercise in grammar or spelling, consisting of a list of words (mostly verbal forms) beginning with the letter *y*. Such texts have also been found at Babylon and published by Pinches.

Further, there is a deed for the transference of a house left as a legacy (no. XV). A certain Manimu and his son append their ' hand ' in witness.

Important also are certain long lists of personal names followed by those of the cities from which these persons came. The lists were probably prepared for purposes of land-taxation, and throw interesting light both on the onomatology of the Ugaritians and upon their political and commercial contacts. The names are partly Semitic and partly Hurrian. The former are distinguished by their resemblances to the nomenclature of the Western Semites, or Amorites, and the South Arabians, especially noticeable in the tendency to add the termination *-ān(u)* to hypocoristica, as in the names of the Hammurabi Dynasty. The latter are often identical with names found in the documents unearthed at Nuzu. The toponyms include Byblus (*G-b-'l*, the *Gebal* of the Old Testament), Wassugani, capital of Mitanni (*U-ś-k-n*) and Hobah, near Damascus, mentioned in Genesis XIV, 15.

One of the smaller texts (no. II) contains what appears to be the ritual for the conclusion of a political alliance between the people of Ugarit and certain surrounding nations, among whom mention is made of the Hittites, Hurrians, and Subareans, as well as of the inhabitants of Alše (the classical Arazene, or perhaps Alashia = Cyprus ?) and of Dadmush (?). They are described as peoples who ' agress, oppress and

distress '. The text presents the same virtually, addressed separately (to judge from the verbal and nominal forms) to women and to men. It is distinguished by the fact that every salient phrase is repeated thrice in synonymous terms and that each paragraph concludes with a reference to offerings presented to the deities. The text therefore represents, in all likelihood, the text of a protocol to which the men and women of the city were solemnly sworn. What makes this conjecture the more plausible is that each paragraph ends with two mysterious words which may be rendered ' here is the wild ass ', and from documents discovered at Mari we learn that the expression ' to slay a wild ass ' (the same word is used in the original) was a regular expression for ' to conclude a political agreement '.

Interesting also is a short letter addressed by a certain Iwri-zar to his friend P-l-s-iya (vocalization uncertain) enquiring after the true circumstances of two men who have complained that ' the hand of God ', being ' vehement like death ', has touched them adversely.

There is also an interesting list of temple-officials, from which we learn that the sanctuary maintained a regular staff of masons, smelters and smiths.

Before passing on to describe the long mythological poems, a few words may be said concerning the documents written in Accadian and in syllabic script.

One of these is a commercial document dealing with transactions in *argaman*, or purple-dyed material. This may have been a regular article of commerce between the people of Ugarit and those of Cyprus, which lies almost opposite, for it is noteworthy that even in the days of the prophet Ezekiel (XXVII, 7) *argaman* was regarded as the special product of ' the coasts of Alashia-Cyprus '.

Other documents written in Accadian deal with transfers of property and with the release of a slave respectively. From the latter we learn the interesting detail that it was part of the ceremony of manumission to pour oil upon the slave's head (*tabak šamna ana qaqqadiša :* RS. 8208, 7-8).

There are also fragments of a well-known and standard Sumerian-Accadian syllabary.

Of the texts written alphabetically in non-Semitic speech it is difficult to speak, since the language in which they are composed is as yet imperfectly understood. One of these documents contains a list of deities whom Hrozny has recognized as members of the Hurrian pantheon, mentioned again in various texts from Boghaz-Kewi. This fact, taken

together with certain linguistic phenomena, has established the conclusion that the language is akin to Hurro-Mitannian. Several words found in Hurrian texts (such as *arte* ' city ', *awari* ' field ', *turuḫ-* ' male ', *ašte* ' woman, female ', etc.) have already been recognized, and it has been found possible, on the basis of the alphabetic spelling, to clarify many points of Hurrian phonology somewhat obscured in the rough and ready syllabic script in which all Hurrian texts hitherto known are written. In the elucidation of these texts the work of Speiser (on phonology), Friedrich (on representation of sounds) and Brandenstein (on vocabulary) has been especially noteworthy. The present writer has also contributed to the subject by an analysis of the inflexional forms to be found in the text and by various combinative suggestions designed to elucidate the meaning. It is his intention shortly to publish a fuller study, correcting previous errors of his method and attempting to carry the investigation to a more decisive issue. At the present moment, however, little can be said beyond the fact that the language, though undoubtedly of the Hurrian family, is not absolutely identical with the Hurrian spoken, or written, at Boghaz-Kewi. It possesses its own dialectical peculiarities.

Concerning the nature of the language, it is apparent that the personal subjective suffix is *-š*, that the impersonal (' neuter ') plural, both nom. and acc. ends in *-n*, that the verbal noun is formed by the suffix *-r*, and that a verbal suffix is *-žnnk*. The enclitic *-m* marks the copula, and *-ḫ* is the termination of demotica. Beyond this, it is scarcely possible to go.

One of the texts is a long litany, in which address is made (always in almost identical terms) to a number of deities, grouped together in a single all-embracing formula at the end. Most of these deities are known members of the Hurrian pantheon, such as Teshub of Ḫallab (Aleppo), Allanis, Iršappi, Imhu-Nikkal, Nupatik, Pisasaphi and Šimige. Mention is also made in this text of a certain *'-n-t a-m-r-n* who is probably the goddess ' Anat, the Amorite '.

Two other of the non-Semitic texts are almost identical with one another in phraseology, except that each contains different proper-names. They probably commemorate offerings. Another contains a long list of peoples, repeated several times, and interspersed with words which appear to be Semitic. The present writer believes that this is a protocol like the Semitic treaty of alliance mentioned above.

Considerable help in the elucidation of this language may be found in a judicious use of a trilingual glossary found at Ras Shamra, the three

columns of which are respectively, Sumerian, Accadian and an agglutinative non-Semitic tongue resembling Hurrian.

An attempt to elucidate the new tongue has recently been made by Bork, one of the pioneers in the interpretation of Mitannian.[1] In the opinion of the writer, however, this attempt proceeds from an entirely unfounded basis and pursues a course more distinguished by ingenuity than by sobriety. This is also the opinion of other more competent authorities.

Turning now to the long mythological poems, we shall first describe their contents and then say a few words about their general character and import.

First and foremost comes the cycle known as ' The Lay of Baal '. The theme of this cycle is the rivalry which exists between the two gods Aleyan-Baal (*i.e.* ' Baal the Puissant ') and Mot (*i.e.* ' Death '). The rivalry involves a continuous struggle for dominion over the earth, each alternately defeating the other. The several tablets which belong to this cycle describe the various phases of this struggle.

On the one hand, there are passages which describe how Aleyan-Baal, with the help of his sister and consort, the war-goddess ' Anat, succeeds in vanquishing Mot and banishing him from the earth. On the other hand, there are passages (Syria, xvi, 247–266) which describe how the supreme god El, whose favourite Mot is said to be, lures Baal to his doom whilst out hunting, and keeps him languishing in a marsh whilst all fertility and vegetation vanishes from the earth.

The myth really symbolizes the alternation of the wet and dry seasons in the Syrian year. Baal represents Rainfall and Fecundity, whilst Mot represents Drought and Sterility. Indeed, the nouns ba'al and mot are used respectively in these two senses in Arabic and later Hebrew usage.

In all likelihood, these texts were not purely mythic in character, but were the *libretti* of sacred pantomimes acted, as in many parts of the world, at the seasonal festivals. On this hypothesis, the several incidents of the myth would be projections from ritual actions, and by probing beneath the ostensibly mythic surface, it will be possible to reconstruct in large measure the ritual of these proto-Hebrew festivals.

The results of such an enquiry are surprising. We find that the Ras Shamra festivals exhibit that same ' ritual pattern ' which has been

[1] Das Ukirutische : Die Unbekannte Sprache von Ras Schamra. Die Grundlagen der Entzifferung. Von Ferdinand Bork. (Mitteilungen der Altorientalischen Gesellschaft, xii, 1, 1938).

detected also in Babylonian and Israelitic cultus, and which has been postulated, by Professor S. H. Hooke and others, as the common basis of seasonal ceremonies throughout the ancient Near East. The essential elements of this pattern are: (a) a battle-royal between two rival powers, each claiming dominion over the earth; (b) the formal installation of the victor as king; (c) the erection of a new habitation for him; (d) the celebration of a seven-day festival.

We need not accept all of the conclusions of the ' ritual patternists ', nor endorse the superstructure of fancy which they have reared upon their data, in order to see that the essential basis of their theory is indeed confirmed by the Ras Shamra texts.

Other tablets, not themselves part of the Lay of Baal, but doubtless associated with the same ritual ceremonies, fill in the picture.

One of these (SYRIA, 1935, 29–45) describes a battle between Baal and his confederates against the angry forces of the sea and the river. The latter are defeated.

The point of this myth is clear enough. Baal is the god of the rain. Sea and river are therefore his rivals since, by affording an alternative source of moisture to the parched soil, they constitute a challenge to his absolute sovereignty over the earth during the autumn months. Accordingly, before Baal can assert that sovereignty, he is obliged to ' put them in their place '. This is what our text describes. The Mesopotamian myth of the great god's victory over Apsu is a direct counterpart. Transferred to Yahweh (Jehovah), the myth reappears in the ninety-third psalm and in the final chapter of Habakkuk.

Another tablet (SYRIA, 1933, 128–151), this time definitely interspersed with rubrics, contains a series of mythological extracts recited as an accompaniment to the performance of various ritual acts. In this text, we hear first of a rite wherein sacred women went down to the seashore, filled basins with water and then returned to deposit them (or pour them out ?) in the temple. This is a rain-charm pure and simple. Lucian, in his treatise ' On the Syrian Goddess ', described it as taking place twice yearly at Bambyke, whilst the Jewish Mishnah records it as a regular part of the ceremonies at the Festival of Ingathering in Jerusalem. Next, we hear of a rite in which a kid is to be seethed in milk, and in the Pentateuch we read of the same rite as expressly forbidden to the Israelites, in connexion with the ceremonies of presenting first-fruits to the deity (Exodus, XXIII, 19; Deut., XIV, 21). Then is mentioned the erection of certain *cathedrae* (*m-š-b-t*) for the gods, followed by the recital of a hymn beginning with the words '*I am*

jealous for the gods '. This links up with the vision seen by Ezekiel, just before the New Year festival, in the courtyard at Jerusalem. There was erected, we are told, at the gate of the temple, a *cathedra* (*m-š-b*) dedicated to the ' Idol of Jealousy ' (Ezekiel, VIII, 3). This is followed by a lament, on the part of the sacred women, for the discomfited and emasculated Mot, and here again we find an echo in Ezekiel's statement (VIII, 14) that part of the heathen ritual which he beheld consisted in a lamentation for Tammuz (*i.e.* the slain god, here equivalent to Mot) on the part of certain sacred women. Lastly, we read in this text of a sacred marriage, such as recurs in corresponding Mesopotamian rituals, followed by the birth of seven sons, the greatest of whom, destined apparently to restore the fortunes of men, is Shaḥar, the god of the Dayspring (Hebrew *shaḥar* ' dawn '). Here, once more, we find a parallel in the heathen ritual described by Ezekiel, for we are there told that part of the ceremonies consisted in a ritual act wherein men turned to the East to greet the rising sun.

Thus, it is apparent that our Ras Shamra text affords the prototype of New Year rituals still surviving in Jerusalem in the 6th century B.C. The parallelism strongly supports the contention that the Ras Shamra documents are the *libretti* of sacred pantomimes, behind which lies the picture of proto-Hebrew seasonal ritual.

Belonging to the same cycle are a series of extracts, recently published under the convenient title of *La déesse 'Anat* (Geuthner, 1938) which describes the exploits of the war-goddess in avenging the discomfiture of Baal, and her successful efforts to bring him back from exile and install him as king of the earth. Several points of similarity with the Syrian Adonis-cult, hitherto known only from late Classical sources, herein make their appearance, and it would seem that, as in other parts of the world, the combat against the coterie of Mot is here historicized into the commemoration of an ancient battle between the people of Ugarit and certain hostile neighbours living in the Orontes Valley.

Of quite a different order is the so-called 'Legend of Keret'.[2] This legend describes a battle between the forces of an ancestral hero called Keret and a certain unnamed adversary. The text has received especial attention in view of a theory advanced by its first editor Virolleaud to the effect that the adversary was named Terach, and that the names of his wife and mistress were respectively Sin and Nikkal, two deities associated with the city of Haran, from which the Biblical Terach,

[2] *La Légende de Kéret* (Geuthner, Paris).

father of Abraham, is said to have migrated into Canaan. Virolleaud therefore thinks that our text may contain an early Canaanite account of the Terachid invasion of Palestine. This theory, sponsored also by Dussaud and others, is, however, now seen to rest on a false interpretation of certain words in the text. The present writer, who formerly supported this theory at some length, renounces his previous position. *Dies diem docet.* The word *teraḥ* does not mean 'Terach' in the crucial passage, but is a common noun denoting 'bride-price' (Accadian *terḫatu*), whilst the words rendered SIN and NIKKAL have quite a different sense, let alone the fact that in Mesopotamia the former is a god, not a goddess! Similarly, Virolleaud's detection in this poem of the names of ASHER and ZEBULUN also rests upon a false exegesis. Thus, the Biblical connexions of this text disappear entirely under the searchlight of a more exacting criticism, and the oft-repeated statement that Terach is mentioned in the Ras Shamra texts is shown to be inaccurate. The merit of this demonstration rests in the first place with Albright, though this does not mean that his own explanation of the passages in question need necessarily command assent. The historical basis of the Keret-text cannot yet be determined, and though most of the philological difficulties can now be removed, it is still impossible (especially since portions are still to be published) to seize the connected sense.

Another important text is that entitled 'The Legend of Danel'.[3] The story is as follows :—

An ancient hero Danel has no son. He therefore prays to the supreme god El to grant him one, promising that the child will serve the deity in his temple (cf. the vow of Hannah regarding Samuel). Through the intercession of the god Baal, this request is granted, and Danel begets a son called Aqhat.

The autumn harvest, at the end of the dry summer season, is now drawing nigh, and Aqhat is destined to be lord of the summer crops and to receive tribute of firstfruits in respect of his regal status. This excites the jealousy of the goddess 'Anat who feels that the privilege belongs by rights to her own heir, not yet born. She therefore decides to attack Aqhat, but upon his presenting to her rich presents and undertaking to relinquish his claim, she is persuaded to desist from this course. Nevertheless, for some obscure reason, 'Anat's henchman Yatpan renews the attack and kills Aqhat.

[3] *La Légende phénicienne de Danel* (Geuthner, Paris).

The desolate father appeals to El who vouchsafes an omen proclaiming the future resurrection of Aqhat. This is later accomplished, after various rites of mourning have first been instituted. The whole earth then brings tribute.

The text is in many places extremely obscure, and one cannot lay one's hand upon one's heart and swear that the above summary is correct in every detail. It must be taken, at most, as giving *one view* of the general sense, and the writer is quite prepared to modify it, or abandon it altogether, when more light is available. Only by being quite candid about the provisional nature of our results can any progress be made in this very difficult field of study.

Lastly, there is a short text which appears to be a prothalamium sung at the marriage of a girl named P-r-b-ḥ-š. Into this poem, by way of graceful compliment, is introduced a mythological passage describing how the moon-god Yarikh once wooed and won the goddess Nikkal. The text is especially interesting because it describes in some detail the ceremonies of marriage among the inhabitants of Ugarit.

Such then, apart from a few minor fragments which are too mutilated for general consideration, are the celebrated Ras Shamra Texts. It remains only to add that their language contains innumerable points of similarity with the idiom of the Old Testament, and that at times they recover for us the fuller forms of myths (like those of Leviathan) to which only passing allusion is made in the literature of Israel.

From the point of view of Biblical study, these documents possess extraordinary importance as recovering to us for the first time contemporary evidence concerning the culture of that world in which the constituent tribes of Israel were nurtured. They open up the far-reaching question : How much of the old Testament is really Israelitic and how much of it is merely the survival of proto-Hebrew culture seen through the prism of Israel's distinctive experience ? This, it is safe to prophesy, will yet prove the dominant issue in Biblical studies during the next ten years. But first of all, the Ras Shamra texts will have to be more fully elucidated.

The documents are also important from the point of view of Classical studies. The present writer has elsewhere pointed out that the long mythological texts, based as they are upon seasonal pantomimes, furnish an early Semitic example of that ' ritual drama ' (*sacer ludus*) which certain scholars have postulated as the true origin of Greek drama. All the elements (viz. the Combat, the Triumph, the Sacred Marriage, etc.) which have been reconstructed by Gilbert Murray, Jane

Harrison, F. M. Cornford and others as essential constituents of the Urform are here indeed present, so that a theory condemned in more 'orthodox' circles as fanciful now demands renewed consideration. If this new view is correct, the Ras Shamra texts will take their place in literature as the earliest examples of European drama and they will, incidentally, throw a flood of light upon the origins of the mummers' play which may then be regarded as the lingering survival of this pristine form.

In concluding this survey, one point should especially be stressed, and that is that all results are necessarily provisional. Much that was written in the excitement and imperfect knowledge of the first discovery is already antiquated, and there is scarcely one scholar who is engaged seriously in this branch of study who would not now wish to forget some of his earlier pronouncements.

Another point that should also be made, and this very strongly, is that for the full appreciation of this literature it is necessary that the services of other than purely Semitic specialists be recruited. There is a danger that material which can only be assessed properly through the fullest co-operation of archaeologist, philologist, folklorist, anthropologist and Classical scholar will suffer through too narrow an interpretation and that wider views will be refused a proper hearing through lack of a duly qualified audience which might judge them.

Modern Views on the Italian Terremare

by D. Randall-MacIver

THERE has been for a good many years an uneasy sense that all was not well with the theory of the Italian terremare as propounded by the original explorers. Writers of general works have continued to accept and to pass it on, in perfectly good faith, as if it were a coin of full value, worth the same now that it was thirty years ago. But those who had the best opportunities of knowing were aware that there had been a very serious depreciation. It was strongly suspected that, like the dollar, it had sunk to at least 59 per cent of its nominal value ; but there seemed to be no one who was at once willing and able to state the exact degree of devaluation. Pigorini's staunchest followers, of whom the last survivor died a few years ago, began to admit that there was a considerable percentage of loss ; while more impartial outsiders wondered whether the terremare coin had not become totally valueless.

It was a long while before suggestions of this kind were made quite openly. A certain conservatism, coupled with a proper respect for great names and a great tradition, deterred scholars of my generation from being too abrupt or too brutal in breaking with the past. But now the time has certainly arrived for a frank revaluation, and it is perhaps better that this should be made by one who has viewed and sympathized with the whole development of the last half-century, rather than by some more unsparing iconoclast of a younger school. In this latter class I may without offence venture to include Säflund, a Swedish scholar whose recently published volume[1] has impelled me to put these remarks on paper.

The theory which I am discussing is no doubt familiar in outline to most readers of Antiquity. Few of these however will have at their disposal a complete file of the *Bullettino di paletnologia*, in which Pigorini at frequent intervals from 1875 onwards expressed his developing opinions. The Terremare are well described by Peet in his still

[1] G. Säflund, *Le Terremare*, a work written in excellent Italian and brought out as one of the publications of the Swedish Institute in Rome ; published by Gleerup (Lund) and Harrassowitz (Leipzig), 1939. To be reviewed later.

authoritative work (*The Stone and Bronze Ages in Italy and Sicily*, Oxford, 1909), but the whole doctrine of Pigorini is given in its clearest and most orthodox form by Modestov in his *Introduction à l'histoire romaine* (Paris, 1907). Modestov accepts Pigorini's views as regards the Bronze Age and supports them with an eloquence which is very impressive. It is only at the beginning of the Iron Age that he parts company with Pigorini and refuses to accept the genealogy which would derive Villanovans from Terramaricoli.[2] In my own first volume on the Iron Age (*Villanovans and Early Etruscans*, Oxford, 1924) I made a similar break with tradition ; and it is symptomatic of a modification in their point of view that the same Italian scholars who fiercely protested against Modestov received my work without protest and even with approval. Opinion was clearly changing fifteen or twenty years ago ; the change has been still more marked since then. Now it is not merely the validity of the genealogical tree of the Roman which is attacked, it is the whole theory including the conception of the *Terremare* as the central pillar of Bronze Age culture in Italy. It will be well to give in a few lines a résumé of the Pigorini doctrine. At the end of the Stone Age Italy is found to be inhabited by a homogeneous people of Mediterranean origin, living in round huts of wattle and mud and using the rite of inhumation for the burial of the dead. This homogeneity is broken by the arrival of lake-dwellers, who make pile-dwellings, like those of Switzerland and Austria, in all the lakes of northern Italy from Lombardy in the west to Venetia in the east. Their civilization is at the chalcolithic stage ; bronze is beginning to make its first appearance but is still rare. The lake-dwellers are succeeded, but only in the eastern part of the above-mentioned area, by an apparently new people who have the curious habit of building pile-dwellings on dry land, sometimes even on hills. Their burial custom is cremation, so that they are sharply distinguished both by their habits of life and by their religious practices from the Mediterranean aborigines. It is inferred that they are immigrants or invaders from beyond the Alps. Their civilization is characterized by a free use of cast bronze, and it is maintained that they were the pioneers of bronze-working in the peninsula. From them, it is said, the knowledge of this craft spread all over Italy. At the close of the Bronze Age their settlements, which were in the eastern valley of the Po, especially round Parma Reggio and Modena,

[2] *Terramara* is a word coined from the dialect form *terra marna* ('dark earth'). As it is derived from two separate words the correct plural is *Terremare*. The inhabitants of *terremare* are called in archaeological jargon *Terramaricoli*.

disappear. It is regarded as uncertain whether they were burned by themselves or by their enemies; Modestov holds that they were destroyed by new invaders. Next—and this is the most sacrosanct part of the theory—the Terramaricoli leaving their homes made a long trek, or perhaps a conquering march, which brought them by devious routes to the lower Tiber. In these new surroundings they founded new settlements, no longer in the style of pile-dwellings except for their linear town-plan, in Tuscany and on the Alban hills. Moving down from the Alban hills they founded Rome with a ground-plan like that of a *terramara*, and became the ancestors of the Romans. Finally their civilization, which by this time had reached the stage of the Early Iron Age, developed into what we recognize as Villanovan, and the Villanovan in turn became a powerful influence spreading over every part of Italy.

Modestov, as I have already said, dissents from this last extension and refuses to identify Villanovans with Terramaricoli, but up to and including the founding of Rome he faithfully accepts the whole doctrine. I, fifteen years ago, rejected the pedigree deriving Romans and Villanovans from the Terremare; but it was not then part of my plan to discuss the earlier period, which belongs to the Bronze Age. I have now therefore to say how much, if any, of the first part of the Pigorini theory should, in my opinion, be retained.

First of all then it has long been recognized that the evidence for the supposed trek from the Po valley is of very flimsy character. I have no space to discuss it, so that the reader if he thinks it worth while must test the argument for himself. He must go behind Colini's ingenious pleading and Modestov's eloquence and ardour to examine the actual evidence, which they have in perfect honesty put forward. If he is then convinced I have no more to say on that point; to me and to many others, in Italy and outside, this march has never seemed to be anything but a poetic figment. The entire Pigorini theory indeed was never more than a semi-prophetic forecast, an ideal epic more in the spirit of Vergil than of sober archaeology. To turn it into serious truth discoveries were needed which Pigorini confidently anticipated, but which never occurred. Colini made a gallant attempt to supply some of the most obviously needed links. But he did not succeed and the whole course of subsequent exploration has weakened instead of strengthening the chain. Like many others therefore I reject all idea either of a trek or a conquering march from the valley of the Po to Rome. I have long ago rejected the chimerical idea that this supposed march was prolonged to Taranto.

How much importance then, if any, can we still attach to the Terremare when so much of the original theory has been abandoned ? Are they anything more than a curious local phenomenon ? To use the language of my simile has the coin been devalued even far below 59 per cent ? I would answer that there may still be a small but valuable kernel of truth underneath all the covering of incredible hypotheses. There is still a good deal of reason for supposing that it was the people of the Terremare who introduced the knowledge of bronze-working into North Italy. This view is in no way invalidated by the new importance which Rellini's discoveries have given to the *extra-terramaricoli*, that is to say the contemporary natives of Italy who lived in wattle-huts instead of in pile-dwellings. For the Bronze Age of Italy, at least in the north, is essentially continental and Central European in character ; it is not directly derived from the Aegean. The people of the Terremare may well have been its pioneers. For the present I at least should be willing to retain this not inconsiderable fragment of the Terremare saga.

Mapungubwe

I. The Excavations and Culture

by G. Caton-Thompson

THE story of the discovery of Mapungubwe,[1] which led to its systematic excavation, makes reading as vivid and exciting as any adventure-fiction of our youth.

The site is one of many precipitous sandstone kopjes which rear their almost perpendicular sides some 200 ft. above a valley-floor tributary to the Limpopo, which lies only a mile distant, at the point where the Shashi river, flowing from Rhodesia, joins forces with it. Messina—archaeologically famous for its pre-European copper mines—lies about 50 miles west, and it overlooks the meeting point of Transvaal, Bechuanaland and Southern Rhodesian territory. Fortunately for science, British administration being parsimonious in these matters, the hill lay within the Transvaal border.

The region at the time of the discovery, in the New Year of 1933, was one of exceptional isolation; malaria-riddled in summer, the farms were mainly unoccupied, or used for winter big-game shooting. Lion and elephant abound.

The local natives in language and tradition show a mixture of Shona, Venda and Sotho elements. To them Mapungubwe—the Hill of Jackals—was a place of terror and taboo. To climb it was death; even in speech it was safer to turn away and not to point. It was however not inviolate. A prospecting party had climbed it in 1929, and Frobenius is believed also to have visited it that year, but, as usual, left no record.

But these explorers had not seen the importance of the site. Some half century ago a wild white man (' Der Wilde Lotrie ') lived in half-crazy solitude in a nearby cave. He climbed the hill and found things there. From it he brought back a large earthenware jar, finer than anything the natives knew. And in due course this passed into the possession of a native who, by then very old, related in 1932 this strange tale of the long past to Mr E. S. J. van Graan, who farms some 50 miles

[1] Mapungubwe: Ancient Bantu Civilisation on the Limpopo. Reports on Excavations from February 1933 to June 1935. *Edited by* Léo Fouché. *Cambridge University Press*, 1937. *pp.* xiii, 178, *and* 45 *plates, with maps and diagrams*, £2 10s 0d.

away. Mr van Graan followed the story up energetically ; and aided by a reluctant and terrified native, unable, notwithstanding, to resist the pressure put upon him, and the lure of a reward, identified the hill ; and with his son and three accomplices found a secret ascent up its cliffs, and forced the passage.

We may at this point hope that the native and his tribe have been very handsomely rewarded, since the intrinsic value of the site has been so rich. This we are not told ; but on plate v appears a picture of Chief Tshiwana, successor and reputed descendant of the legendary chief Mapungubwe, clad in the usual disgusting rags of European discards—a pathetic picture.

The hill-top proved to be a stronghold reached by a cleft made scaleable of old by rock-cut notches. Breast-works of dry masonry crowned the summit, and boulders ready to be hurled upon unwelcome visitors. The flattened top measured 300 yards by 40 wide, and bore an immense overload of earth and midden dust and stone, brought up by hand over a long period to form the huts and hearths, floors and granary-platforms of permanent settlement. Potsherds lay everywhere and search in the loose soil yielded the explorers iron tools and copper wire and gold . . . gold the bane of archaeology, responsible for more pitiable destruction of historical evidence than ever followed in the wake of savage armies. Here the vile metal consisted of thin gold plates beaten into sheathing for carved figurines of rhinoceros and other objects, fixed with small gold tacks ; of bangles, beads. And, more important, iron and a fine pottery bowl. These lay with burial remains which, in the circumstances of the excavation, did not survive intact.

Up to this point the proceedings had pursued a well-worn course, known only too painfully by those who followed the archaeological track of destruction left by Hall and Neal and the pioneer tragedians of the Rhodesian ruins. But imperceptible as it sometimes seems, particularly in official circles, the world since those days has become increasingly science-conscious ; the labours of the teaching profession and those who lecture have not been wholly unavailing.

How Mapungubwe was thus miraculously saved from secret destruction, is best told in the words of the editor Professor Fouché, inevitably condensed.

'The five fossickers had realised the schoolboy's dream—they had found hidden treasure. But they were in a dangerous situation; they were trespassers and in possession of property which the law might regard as stolen. There was much debate as to what to do next. Eventually the two van Graans were forced to agree to a division of

the spoils on the spot. The gold was roughly divided into five equal portions. This was the most critical moment in the history of Mapungubwe. The site was so remote and lonely that unscrupulous treasure-hunters could easily have ransacked it completely and got away with their loot. Fortunately the van Graans were men of education, and young van Graan when a student at the University of Pretoria, had become interested in the story of Zimbabwe . . . He remembered regrets expressed in lectures on the subject that at Zimbabwe no human remains had as yet been discovered, since such a find might well solve the mystery.[2] Here at Mapungubwe they had found what looked like Zimbabwe gold in conjunction with a human skeleton. He realised the scientific importance of the discovery and resolved to consult his old professor. Thus it came about that the writer, some weeks later, received from Mr J. C. O. van Graan specimens of the gold plate, bangles and beads found with the skeleton . . . It was necessary that every effort should be made to recover all the contents of the rifled tomb. With the cordial cooperation of the University of Pretoria, the writer was able to organize an expedition in order to recover the treasures already scattered, and to examine the site. The legal problems arising out of the questions of treasure-trove and ownership were very obscure. These were immediately investigated by Advocates A. A. Roberts and E. C. Niemeyer, while search was also made for the owner of the farm on which Mapungubwe is situated. Police headquarters were approached for authority to deal with the possessors of the treasure-trove should necessity arise . . . Sub-Inspector Kruger, C.I.D. accompanied the expedition . . . The discoverers were traced and persuaded to hand over all their finds at a valuation, and further to make over their discoverer's rights to the University. The tact displayed in these delicate negotiations by Mr F. V. Adams places the University and the scientific world under a deep obligation to him. Sub-Inspector Kruger was not required to intervene officially; his presence and moral authority sufficed '.

The passage has been quoted at length because it is a model of vigorous action—alas rare in archaeological emergencies—taken swiftly, comprehensively, inexorably, to save a scientific trust belonging not to South Africa alone but to the world. We feel sure that a non-gold-bearing site of equal scientific importance would have been similarly treated.

Action thus initiated by private enterprise was sustained and developed. The University of Pretoria acquired the excavation rights and manfully shouldered the heavy preliminary expenses; and at Easter 1933, a scientifically selected party went up to Mapungubwe for a week's test, survey and report. The skirts of the hill were found to be rich in material washed down the cliffs; vast settlement-deposits lay in the vicinity. Professor (then Mr) van Riet Lowe, one of the party, in a brief but excellent report, confirmed the scientific value of the site and urged its excavation. The Prime Minister, General Hertzog—an

[2] The reviewer classes ' mysteries ' along with gold, as a pest in archaeology. The Zimbabwe ' mystery ' was dispersed some years after its gold (*i.e.* in 1905). Neither may be said to have existed since.

example yet to be followed by his English counterparts—declared the undertaking ' a matter of national importance ', and General Smuts gave it his energetic attention. In June 1933 the Union Government decided to buy the farm on which Mapungubwe lies[3] and to support financially the excavations. The University of Pretoria, in recognition of its exertions, was granted a five years' concession.

In London that summer General Smuts discussed with me the implications of the find and the best procedure in regard to it. I urged the competence of South African archaeologists—Mr van Riet Lowe in particular and Mr Neville Jones—to undertake the excavations, at a time when many were discussing more experienced overseas names : and I stressed the ultimate benefit to Union archaeology of building up through field-work its own tradition of responsibility.

Moreover the Limpopo finds seemed to me almost certainly to be another instance of medieval or later native culture, allied to, though not yet proved identical with, the Rhodesian material. But since the unanimous findings of two successive British archaeologists of authority concerning the latter had failed to overcome South African prejudice against acceptance of a native origin and comparatively late date, and had given rise to much abuse and anger, it seemed to me better that the verdict, similarly unacceptable as it was bound to be, should come this time from South Africa itself.

A preliminary test in July 1933 under the direction of Professor Fouché was made and 23 ft. of occupational débris found to overlie bedrock. An adjacent site, Bambandyanalo, some 500 yards south, later to become of special importance, was discovered, and a general air survey instituted.

Mapungubwe thus saved was now ready for systematic excavation. This began in May 1934 and continued for two months under the direction of Mr Neville Jones and Mr J. F. Schofield, with Mr van Tonder as ' non-technical assistant '. The third and last month was spent prospecting ruins in the neighbourhood. Meanwhile Professor Lestrade, whose ethnographic studies of the Ba Venda, Suto-Chuana and other tribes confers on his opinion particular authority, undertook the difficult task of investigating the traditional connexions between the local natives and Mapungubwe. His contribution (part VI), though not in many ways conclusive in establishing a direct connexion, reveals the ethnic history of the area as a complex affair in which all that seems

[3] The figure rumoured was a large one (£12,000). Certainly nothing comparable has before been done on this scale to safeguard archaeological field-work.

certain is that Shona-Venda clans were conquered, possibly 150-200 years ago, by Sotho elements. Venda, Lemba, Shona, Sotho and Thonga-Shangaan individuals were questioned, and their ability to identify and interpret objects from the old site tested; comparisons were made between the ancient article and its modern representative. Beads and pottery clearly show many points of close similarity, though differences, inevitably, also. Professor Lestrade, who accepts the culture of Mapungubwe as 'Bantu' is inclined to attribute much of its inspiration to Lemba influence. He rightly emphasizes that gold was formerly regarded as a metal of special value only in those areas with Arab contacts; and he would ascribe to the Lembas an attenuated Arab strain and cultural tradition, sufficient nevertheless to account for the fine gold-working of the ancient sites. This negro-transmitted Arab influence was the view I expressed eight years ago; and my archaeological observations in the Hadhramaut in 1937–8 have strengthened it. The beads I found there in tombs covering the last few centuries before the Christian era are totally unlike the Zimbabwe-Mapungubwe series, and contradict any attempt which might be made to overdate to that extent the South African sites. But with them were quantities of small copper tacks, indistinguishable I think (without comparison) from those of gold or copper in South African deposits; they point at least to a common technique in metal sheeting which the disparity in age cannot invalidate.

The volume is an unusually composite one, even in these days of team-work, for it includes the finds made before 1934 (part I) and overlaps at the other end (parts VII–IX) Messrs. Jones and Schofield's work, which ended in September 1934, to be continued since by other excavators. A review of Vol. I therefore which represents the start only of continuous work, that has since yielded a large amount of fresh material and information—enveloped if rumour may be trusted in some remarkable theories—is not an easy task, and certain judgments may be revised or modified in the light of the second volume. We know from an article on Mapungubwe by Mr van Riet Lowe in ANTIQUITY, September 1936, that Captain Gardner, who succeeded Messrs Jones and Schofield as excavator, believes the Bambandyanalo site to be far older than Mapungubwe with its gold burials, and to be at the start in no way connected culturally with it. We therefore keep a critical watch for any evidence, stratigraphical or cultural, in support of such a view in the present volume.

Detailed information about the dig by Mr Neville Jones, forms

part II. Five long trenches, off-sets and tests were cut in the hill-top deposits. These are plotted in red on the admirably clear coloured plan made by Mr van Riet Lowe in 1933. The trenches total about 250 yards in length, and though narrow (5 ft.) were unlucky to miss the grave area beside the original van Graan burial, which was found later and cleared by the ' non-technical assistant ' Mr van Tonder, pluckily working on alone through the malarial winter of 1934–5, after Messrs. Schofield and Neville Jones had finished. One trench lies only 10 ft. from an outlying grave. This is not to imply that the 7 ft. or so of occupational débris disclosed in these trenches is less scientifically valuable than a cemetery. Indeed it alone may provide the stratigraphical and relative dating evidence essential to the inquiry ; and undoubtedly the excavators, faced with an overwhelmingly large choice of productive areas, were right in choosing it, instead of concentrating, as less scientifically restrained workers might have been tempted to do, on the discovery of graves. Nevertheless it is extremely regrettable that the investigations of adjacent settlement and graves were not both handled by the same excavators. Not only would it have provided Messrs. Neville Jones and Schofield with a wider basis for their conclusions, and comparative data for their Bambandyanalo burials, but it would have averted the misfortune, as it seems to me, of separating in publication the two aspects of interrelated evidence.

In this first volume we are given a long and startling anatomical section (part VIII) from Dr Galloway on eleven of the burials (including the van Graan find, M.I), but there is no corresponding archaeological description or plan, grave by grave, of these burials. All one can deduce after laborious search in several sections, is that three of them (additional to M.I) contained a considerable amount of gold. Some of this is reproduced in three costly colour-plates. We would much rather have had, to complete Dr Galloway's physical analysis, the excavator's detailed report, accompanied by line or photographic plates of the grave-goods, and telling us, what we *must* know, *what sort of deposit the graves were sunk in and covered with* ? occupational or not ? We shall, of course, expect these details in the next volume.[4]

To return however to consideration of what we *have* been told, we find that Mapungubwe hill-top, thus extensively trenched and admirably documented with cross-sections, yielded thick kraal strata with ' cement ' hut-floors from which iron tools and weapons ; copper wire

[4] That the material was occupational is stated by Mr Schofield in a paper dated 1938. *Transactions of the Royal Society of South Africa*, XXVI, part IV.

over fibre cores, bone needles, awls, etc., ; pottery, slag and tuyères ; beads in glass, gold and shell, were recovered. To complete the resemblance to a Rhodesian ruin inventory, two scraps of late Sung celadon (12th to 14th cent.) were found in different trenches—not on bedrock. The deposits were culturally homogeneous throughout, and the excavators rightly reject the celadon as absolute dating evidence. It merely provides a *terminus ad quem*.

Excavations at the base of the hill yielded similar results, but extended the information about huts and stone walling, from which it is deduced that the earliest settlers occupied the lower ground, and somewhat later moved to the hill-top.

Finally the important site of Bambandyanalo, a vast midden area, was tested in a trench 50 ft. long by 4 ft. wide, which touched bedrock at 15 ft. Children's graves were unearthed on its lower slopes. From these excavations was recovered a fine series of pottery, clay figurines human and bovine, ivory bracelets, iron, copper, tuyères and crucibles and beads of Mapungubwe type.

Mr Jones, supported by expert's reports reviews the different classes of objects found. Gold ornaments resemble the Rhodesian ruin gold and appear to be of the same period and source : there was no evidence of local manufacture. Iron on the other hand was smelted and forged on the spot. (The weapons and tools illustrated include most Rhodesian types). Copper was largely used as wire for twisting up over fibre into anklets and bracelets. Mr Jones says a single example of bronze was found (tin content not stated) in the form of a bucket handle, but Professor Stanley in a later section refers to ' bronze bangles '. (These were also found at Zimbabwe.)

The metals are dealt with by experts in part v. Dr Weber and Mr Pearson, who pronounce metallurgically and technically on the gold, find it to be of high purity and consequent softness. Dr Weber believes the beads, including the perforation, to have been made as castings. This is not the view of Mr Pearson or Professor Stanley. In regard to the sheeting Mr Pearson comments, ' to produce gold plate of remarkable uniform thickness, free from holes and of considerable area, necessitated much skill and time, and it would tax the capability of a modern gold beater to do this with what were no doubt very primitive tools. Gold plate on the " Sceptre " skeleton is $\frac{1}{1000}$ to $\frac{5}{1000}$ in. thick.'

Professor Stanley, experienced in Rhodesian ruin material, reported on the other metals and slags and states ' the specimens submitted are such as would be made by simple native methods of smelting : while

two pieces of the metal product similarly exhibit the characteristic features of such material from other ruins, including Zimbabwe, as well as of modern native-made iron. These features are lack of homogeneity, inclusion of slag, and partial carburization, with corresponding variation and quality' . . . 'In conclusion I would only observe that the Mapungubwe material appears to present just the same general characteristics as that from similar sites in Rhodesia'.

Beads are dealt with in a contribution (part IV) from Mr Beck, whose wide experience from every land and period makes his pronouncements of unrivalled authority. Altogether about 2000 beads were examined. These include packets from the hill-top trenches, grave area and graves; from the lower settlements; from Bambandyanalo midden and graves; and from other ruins in the Northern Transvaal explored by the excavators. Sufficient material is presented therefore for detailed comparative work between the different localities; this, together with the pottery, will be invaluable should questions arise concerning their relative ages and interconnexions. A short summary of complicated material is impossible; nor can the evidence be intelligently followed until the reader has spent exasperating time pencilling against the otherwise helpful bead chart the provenance of each of the thirty-four parcels analysed under alphabetic labels A^1 to X^2. Editorial supervision might have spared us this. The evidence may be reduced to three main questions: first the relation to Zimbabwe; second, the probable age or ages; third the relation between Mapungubwe and Bambandyanalo.

The first and second are answered by Mr Beck: 'My impression of the Mapungubwe beads is that they come from a very similar civilization to the Zimbabwe ones, but that they are not quite so early as the bedrock layer'. He emphasizes however the very different *proportions* of each type in the two sites. For instance an opaque Indian-red bead, 6a of the Zimbabwe series, is there only 0.5 per cent of the total, whereas at Mapungubwe it is 19·5 per cent. Other disproportions are equally striking, but their significance is speculative. Nevertheless of eight different sorts of tiny cane beads listed from Mapungubwe and Bambandyanalo (marked A^1 to Q^5 in the chart p. 112) seven are found also at Zimbabwe in bedrock layers. The interconnexion of the hill-top culture with Bambandyanalo at its base is indicated by the fact that, of these same eight, all present at Mapungubwe, five were found also at Bambandyanalo; but these proportions may change when the past four years' work is published. We know from later information than that under review that a large cylindrical green or blue wound

glass bead (barely mentioned in this report) has turned up there in quantities, and was present too with a burial in the Mapungubwe grave area. Its importance is manifest, for the moulds in which it was locally made by reusing apparently the small blue cane beads have been found.[5] It will be of interest to see how the next official publication interprets this discovery in terms of relative dating.

The present bead section would have greatly benefited had it been possible to distinguish, not only which beads came from which site, but at which level at that site : through lack of that facility for which the excavators are probably not responsible critical analysis is defective. A useful list of beads found at bedrock on Mapungubwe hill is however given.

The beautifully illustrated publication of the pottery (part III) by Mr J. F. Schofield is a masterly contribution to African archaeology in general and South African in particular. It is logical in presentment, exact in description, and legitimately bold in deduction : it sets a standard for South African wares well up to the exacting level of modern professional archaeology.

In a short introduction Mr Schofield's general attitude to his subject is set out, with emphasis on the continuity of traditional technique and pattern in pottery making amongst the Bantu ; since women are the potters who, when one tribe overruns another, will mingle in their craft the newly-acquired tradition in their old.

Despite the admonitions of the physical anthropologist he uses the word ' Bantu ' in a cultural sense to embrace his materials ; and rightly in my opinion, for a certain underlying unity of social and material culture, superficially diversified though it be, *is* exhibited by different tribal speakers of the Bantu tongues. ' Women make the pots as well as teach the babies to talk ', as Professor Myres once wittily put it in a contribution to this subject. The word thus used in a combined linguistic and culture-complex sense, is surely no more objectionable than the word ' Arab ', which includes similarly diverse ethnic types, held together on the material and psychological plane by a fundamental continuum of thought and culture.

The pottery, then, of Mapungubwe and Bambandyanalo is ' Bantu ', and has affinities with modern Bantu, not with Hottentot or Bushman, wares. Further, though more abundant and perhaps consequently more varied, much of it has a very close resemblance ' almost amounting

[5] *Bantu Studies*, December 1937. ' Beads of the Water ' by C. van Riet Lowe.

to identity ' to what has been found at Zimbabwe and other Rhodesian ruins, and which lately has been recognized in several other places in Southern Rhodesia.[6] After close attention to the nineteen excellent plates of drawings, I concur.

An important classification of the wares into three main groups follows. M1 is a fine ware usually black to grey with or without wet-incised decoration of hatched triangles or other geometric figures. The forms are restricted to simple bowls of four main types, and shouldered pots with high necks. One would however like to know in what proportion red and black wares of class M1 were found, particularly as our curiosity is aroused by a colour-plate of a lovely shallow bowl with haematite burnish and ledge-lugs found with a burial (no. 11) on Mapungubwe hill which Dr Galloway, although the bones are evidently fragmentary, pronounces confidently to be Bush or Boskopoid.

This M1 ware, ranges back to bedrock level on Mapungubwe hill and was found also at Bambandyanalo midden, but if at bedrock is not stated. It is the B and B2 of my Zimbabwe classification[7] as far as the black ware is concerned.

M2 is a mainly coarse brown ware, burnished or matt. Diagonal or hatched bands are roughly made with comb, wire bangle, triangular stylus or other instrument. Some decorations are scratched after firing. The forms are varied, and include spouts, pedestals, ring-bases and plastic knobs and ornament. The ware is associated with M1 at bedrock level on the Hill, and at Bambandyanalo becomes the ascendant class. At Zimbabwe my class A is certainly included in this more comprehensive gathering.[8]

Class M3 includes provisionally all pottery not apparently of local fabric, but derived from other tribal elements. It includes the polychrome ware of Khami and other Rhodesian ruins—my type D.

Mr Schofield's conclusions, based on a quantity of carefully sifted evidence from native wares past and present, and supported on more

[6] For instance J. F. Schofield, ' Salisbury Commonage Sites '. *S. African Journ. Sci.*, vol. XXIX, 1932. ' Pottery from the Salisbury District ', *Trans. R. Soc. of S. Africa*, vol. XXVI, part IV, 1938. Also from well-stratified deposits at Gokomere.

[7] It lay *in situ* beneath the foundations of the Conical Tower ; in the Acropolis midden on bedrock, 24 ft. below the surface ; and in rare scraps (five only) below the cement floors of the Maund ruins (all at Zimbabwe) but was plentiful above them.

[8] *Zimbabwe Culture*, 1931, p. 25 : ' The vertical distribution is interesting : in the Maund it is almost entirely confined to the sub-pavement stratum. Out of 651 sherds . . . only 4 came above the cement floor '.

general grounds by Mr Neville Jones, are that M1 is of Shona stock; M2 of Sotho stock, and that both contributed their separate streams to the creation of the Mapungubwe culture, where overlap took place. He suggests that Zimbabwe was founded by people of Sotho stock (pottery A.Z=M2) probably before A.D. 1300, who about 1450 deserted it and crossed the Limpopo, to establish themselves around the foot of Mapungubwe kopje. At Zimbabwe they were succeeded by people of Shona stock (pottery B.Z and B2=M1) who continued in occupation until the eighteenth century, spreading the while into the Northern Transvaal, to which they brought their goldsmith's crafts, and, on Mapungubwe hill-top, amalgamated with their Sotho predecessors. Both peoples were cattle herders *and* agriculturalists. A close connexion between the Zimbabwe culture (or cultures) and the two Limpopo cultures is indicated, supported, unanswerably, by pottery, beads, copper, bronze and iron articles, gold beads, gold sheeting, gold tacks.

The validity of this view, to which Professor Lestrade's inquiry amongst the local natives raises no objection, is seriously challenged in the skeletal analysis by Dr Galloway, a member of Professor Dart's staff at Johannesburg.[9] He identifies from the fragmentary material dealt with in this volume, a new non-negro race to which he gives the compound name Bush-Boskop—unfortunately in my opinion, since Boskop man is a unique fossil almost certainly of stone-age date, and confusion may result. A complete dead-lock between the physical anthropologist and the massed evidence of cultural anthropology seems thus indicated; and neither is likely to recede. The problem embraces also the Rhodesian ruins and ancient mines, for there though scanty, physical material is not entirely absent. Such as it is it had the benefit of the late Dr F. C. Shrubsall's examination: though intimately acquainted with Bush and Hottentot material, he pronounced it, in each case, to be negro. Other remains from ancient mine-workings and two imperfect skulls from my Dhlo-Dhlo excavations, were considered to be negro also by Sir Arthur Keith, who stated: 'thus all the remains we have from ancient ruins and mines are of the negro (or Bantu) type. No trace of Arab, Egyptian, Bushman or Hottentot'.

Personally I should find it hard to believe, without overwhelming proof unlikely to be found, that a Khoisan people, plus or minus Boskop, evolved the Mapungubwe culture—it is alien to all known tradition of

[9] Dr Morant's review appended has not been seen by me; nor my contribution by him. Professor Dart himself has always hotly maintained his belief in the non-negro origin and very ancient date of the Rhodesian ruins.

these peoples. Nor is a problem-evading assumption of Bantu culture-adoption any more convincing. I venture to think that a solution acceptable to anatomist, archaeologist and anthropologist will not be found until far more work has been done on an extensive scale, not only on the physical characteristics of Bantu-speaking tribes 'that congeries of Hamitic-Negro peoples, gathered together in the scientific net of linguistics, but escaping capture through its wide mesh which seeks to draw them on board for closer racial dissection';[10] but also on a far longer series of human remains from Rhodesian, Transvaal or other sites, yielding the culture of this very remarkable ancient civilization of South Africa.

Mapungubwe

II. The Skeletal Remains

by G. M. Morant

PART VIII (pp. 127–174) of the volume reviewed* has this title. It was written by Dr A. Galloway and he acknowledges help received from seven research workers in the Department of Anatomy, University of the Witwatersrand, who had previously collaborated with him in an anthropological report on 'Bush' skeletons. The conclusion reached then is that 'generically, peoples called Bushmen, Hottentots and Strandlopers are one', and the term Bush Race is applied to these groups. The work on the new material is remarkable on account of the thoroughness with which it was carried out, and it is also noteworthy because the conclusions reached appear to conflict with those formulated by the archaeologists.

Unfortunately, it cannot be said that the human remains which have received so much attention form an extensive series They consist of fragments of the skeletons of eleven individuals, excluding a mandible of uncertain date which is said to be 'predominantly Negro'. One

[10] *Zimbabwe Culture* p. 8. Is it not possible that unsuspected Bush features lurk in the physical make-up of certain 'Bantu' tribes? Dr Galloway finds, as we all do, what he is on the look-out for, and has reported 'Bush-Boskopoids' from ancient pit-circles at Penhalonga, Southern Rhodesia (*S. African Journ. Sci.* XXXIII, March 1937). Now Dr Wells, from the same School of Anatomy at Johannesburg, reports them in the Mumbwa cave, Northern Rhodesia, in association with pottery, iron and late stone age implements. (*Man*, May 1939).

* See footnote, p. 324.

of the eleven is represented by bones which are so fragmentary that it was decided that no description would serve any purpose. Of the remaining ten individuals, six are represented by skulls (with or without a few vertebrae) and all except one of these are very imperfect, while four people are represented by fragments of the skull and other parts of the skeleton. Six are adult—three female, one male and two unsexed owing to defect—and four immature. It is clear that the bones from Mapungubwe are in a very bad condition. The material was reconstructed to a considerable extent in the laboratory.

Given such a series, most anthropologists would decide that there could be no hope of deducing useful conclusions regarding racial affinities from any of the specimens other than the crania, while the crania could not be expected to give any but very tentative results. Dr Galloway, however, has no hesitation in deducing very far-reaching conclusions from the restricted material. He provides a detailed description of each specimen and gives its racial pedigree without hesitation. The first, for example, is said to be definitely ' a Boskopoid type with some Bush features and no Negro features ' ; the third may ' be diagnosed as a Bush-Boskopoid admixture with a slight Negro infiltration, if features of the Negro adult can be regarded as Negro in an immature subject ', and so on in each case. Tables of measurements are also provided, but these are considered to be of lesser importance. For example, the best preserved part of the third skeleton, which is that of a child about 10 to 12 years old, is the skull, and this is ' so distorted and warped that measurements have no significance, so diagnosis must be based on non-metrical features '.

The descriptions of the bones given relate principally to morphological features of this kind. It is misleading to some extent to call them non-metrical, since many refer to the sizes or shapes of particular parts ; hence they might be expressed by measurements, though it would doubtless be difficult to devise effective ways of expressing some of the features metrically. One objection to verbal descriptions of skeletons made for anthropological purposes is that they are inexact. Anyone wishing to compare the Mapungubwe with another series could not be sure of estimating particular features on precisely the same scales, as it were, as those used by the workers in Johannesburg. Measurements obviously have a great advantage in this respect. The remarks on non-metrical features are necessarily subjective in a way that measurements are not, and this is felt particularly when comparisons are made between different specimens. Judgments are then influenced

by the experience the observer has had in handling the specimens in question and others, and they cannot be adequately controlled by an anthropologist who had not had an opportunity of examining the former.

The study noticed forms part of a comprehensive report on excavated material from a particular site. Archaeologists interested in the new knowledge gained are not likely to read the whole of the section on the skeletal remains. The greater part of it is too detailed, and too burdened with technical terms, to make any appeal to them, and after reading the introduction to the section they will doubtless turn to the discussion and conclusions. These set forth the views of the writer in a concise and straight-forward way, but in fact they take a great deal for granted, viz. the legitimacy of the method of analysis used in the body of the study. The archaeologist can only suppose that this is a highly specialized matter : he may, or may not, be willing to take the analysis for granted and to accept the conclusions presented. The way in which the general question of method is considered by other physical anthropologists may be of some interest to him.

There are two principal ways in which series of bones have been studied for the purpose of disclosing the racial relationships of the people they represent. The first and earlier method is that of the comparative anatomist. He gives verbal descriptions—aided by photographs, drawings and possibly measurements—and his general conclusions will be derived from a comparison of all the principal features of the specimens. It would usually be agreed that this is the best way of deciding whether a particular skeleton represents the modern species of man or not, providing, of course, that the observer had had a sufficiently wide experience of skeletons belonging to this and other species. By merely handling the bones the anthropologist may be able to decide such a question to his own satisfaction, and he may be able to convince his colleagues by a verbal account of the comparisons made, though this is not likely to convey much to anyone who has not handled the material. The experienced observer will be able to appreciate whether all the characters—considered singly or in conjunction—of the new skeleton fall within the range of variation exhibited by modern man, or whether some of its characters fall outside this range, and thus indicate distinction from *Homo sapiens*. An examination of this kind is far more difficult to carry out satisfactorily when it is a question of distinguishing two very similar groups of individuals. There may be no anatomical features which distinguish all members of Group A from all members of Group B : their distributions may overlap for all characters, so that

the distinction depends on small differences between average values. There is every reason to believe that this is the situation found in the comparison of all pairs of races of modern man. There are no known osteological characters for them which make complete separations such as those made by skin colour in the case of certain groups of living people.

The second, and much more recent, method of analysing osteological material depends on metrical descriptions. It became customary for anthropologists to record measurements of bones nearly a century ago, and the records are now quite extensive : they relate far more to the skull than to any other part of the skeleton. Measurements of large numbers of crania are only available for a particular group of characters which aim at giving a description of the sizes and shapes of the skeleton of the head considered as a whole and of all its principal parts. They are necessarily restricted in scope and they give no information regarding many anatomical details. On the other hand, the measurements have several advantages. They are objective records : if taken accurately and in standardized ways, they record exact information which does not depend on the experience of the observer, and hence they can be used precisely for comparative purposes by other workers. Furthermore, the measurements make it possible to obtain reliable estimates of certain characteristics of the populations represented, such as average values and measures of variability, which cannot be obtained in any other way with anything approaching the same degree of precision. The ways in which the metrical data may be used to give information of this kind were not fully appreciated until modern statistical methods had been applied to them. Karl Pearson was the pioneer in this field. A new technique of description and analysis was slowly evolved and, though it cannot be said that the form of this technique has been definitely established yet, still the value of the new approach is sufficiently evidenced by the fact that a majority of the researches in physical anthropology published in recent years employ statistical methods.

Anthropologists must not be divided into those who use anatomical and those who use metrical methods of comparison. In fact nearly all use both methods to some extent, though most definitely incline in the one direction or in the other. As there are two ways of investigating the same problems, it is to be hoped, of course, that they will lead to concordant results : they should be complementary and in no sense antagonistic. Unfortunately, it is quite impossible to reconcile some of the

general conclusions reached by statistical means with conclusions of certain kinds regarding relationships put forward by those who rely almost entirely on anatomical comparisons. For example, Dr Galloway implies that it is possible to assign any individual skull to a particular racial group, and his diagnosis is based almost entirely on 'non-metrical' features. On the other hand, the biometrician asserts that it is usually quite impossible to do this by using the commonly recorded measurements, and that it is unlikely that any others which might be taken, if records as extensive were available for them, would make it much easier to determine the race of an individual specimen. In fact he goes much further than this by declaring that 50 or more adult skulls (not in a fragmentary condition) of one sex must be available before it becomes possible to distinguish with any assurance the group they represent from closely related groups. One advantage of statistical analysis is that an estimate can be obtained of the amount of information necessary to give a reliable conclusion of the kind required. There is no such control in the case of the anatomical analysis, and no guarantee that two workers using such a method will reach the same results.

Anthropology is the study of large groups of people, and statistics is the science concerned with the reduction and comparison of group data in a systematic way. One of the most important characteristics of a population concerns its variation, that is to say the extent to which different individuals composing it differ from one another. The statistical treatment of measurements for them makes it possible to measure this feature, and information so obtained regarding racial communities has led to certain definite conclusions which are not generally appreciated Numerous adequately long series of skulls of various dates, coming from all parts of the world, have been studied, the earliest as yet available being the predynastic Egyptian from Badari. These may be chosen, by taking into account any relevant archaeological or other evidence, so that each represents a single community, as far as can be told. Each may be believed to represent a population such that its members—or the majority of them, at least—had been intermarrying for a number of generations. All series so chosen exhibit very considerable variation, and the majority of them show a remarkably close approach to equality in this respect. The variabilities of a few of the series are found to be exceptionally large, and in these cases it may be supposed that heterogeneous groups are represented. In general it may be concluded that there is no evidence to show that racial communities have become appreciably more variable

in the past 7000 years. The modern populations of Europe show a tendency to be slightly more variable than those of the continent in late prehistoric and historic times, and also slightly more variable than modern populations in other parts of the world, but no stress can be laid on these distinctions. Anglo-Saxons differed almost as much from one another as seventeenth century Londoners did.

When ways of analysing anthropological material are under review, considerations of this kind are of the utmost importance. One great disadvantage of verbal descriptions of bones is that they convey no precise impression of the variability exhibited by a series in the case of the features treated. Descriptions of ' non-metrical ' features are inadequate in this respect, and it may be argued that those who rely on them in making racial comparisons fail to appreciate the importance of a knowledge of the natural variation exhibited by the groups to which reference is made. This seems to be obviously so when modern series are interpreted in terms of descent from palaeolithic populations represented by single specimens. Dr Galloway refers frequently to the type of Boskop man, and his racial diagnoses of individual skulls or skeletons include such labels as Boskop-Bush, Boskop-Bantu, and Bantu-Bush-Boskop. But Boskop man is known only from a very imperfect skull which lacks the facial skeleton. It is very unlikely that he was a perfectly typical member of the group to which he belonged, and our knowledge of the variation exhibited by that group is absolutely nil. We are told that the Matjes River and Zitzikama types are variants of the Boskop, but the evidence is quite insufficient to judge whether these other specimens represent the same population or not. Speculation in terms of the almost-unknown is not likely to be very profitable.

It is most unfortunate that there should be such a great discrepancy between the results obtained by the two different methods of analysis employed by physical anthropologists. The one method leads to definite statements regarding the racial origins of individual skulls or skeletons, and it appears to provide all that the archaeologist could desire. Those who trust in the other method say that it is incredible that so much information can be derived from so little evidence. They should not assert that the anatomical appreciations are erroneous, but they may consider them to be mere speculations which should be recognized as such as long as an effective guarantee of their accuracy is lacking. Some control is obviously needed. On the other hand, the archaeologist is likely to be disappointed if he submits a short series of

human remains—obtained with great difficulty, perhaps—to an anthropologist who trusts principally in metrical comparisons. If there are fewer than 50 skulls or skeletons, the latter can only say that measurements and descriptive remarks should be recorded, by all means, but that it is unlikely that the short series will lead to any definite conclusions regarding the racial relationships of the group represented. The data given will be of such a nature that it will be possible to pool them with any which may become available later for individuals belonging to the same group. After all, the biometrician may point out, the sample represents a large population made up by some tens of thousands, or even millions, of people, perhaps ; and it surely is unreasonable to expect that much information regarding a community of that size can be derived from ten or twenty individuals. The archaeologist must hence expect to get reports of different kinds, according as one species of anthropologist or another examines his skeletal material. In time, no doubt, anthropologists will be able to speak with a single voice.

It is proper that this discursive review of a specialized study should be concluded by a summary of the main conclusions stated in it. The Mapungubwe remains are said to exhibit South African—Bush, Boskop and Negro—traits only, so that there is no evidence of foreign admixture. The skulls are compared especially with a Basuto series representing the modern Negroes of the Transvaal, and it is concluded that the earlier people are ' definitely non-Negroid of the Basuto type, but a stabilized Bush-Boskop group '. At the same time the Mapungubwe series ' represents a homogeneous people, which had been stabilized over many centuries, since the racial features are so constant '. An appendix gives detailed descriptions of the dentitions, and it is concluded that ' all the teeth have a frankly Bush or Boskopoid facies '. The archaeologists conclude that the culture of the people was definitely ' Bantu ', however, and this appears to conflict with the evidence of the people themselves, that is to say of their bones. In the present writer's opinion it would be unwise to attach significance to the apparent discrepancy, owing to the paucity of the physical evidence and to the uncertainty attached to the interpretation of it. Much more abundant material would be required to establish such an unexpected conjunction of race and culture.

Notes and News

WATTLED HOUSES IN SCOTLAND

Exact information about houses in the Highland Zone built of other material than stone is not easy to come by. The houses themselves vanish and leave no trace ; whereas the more durable remains of stone houses and huts, however primitive, are for that reason far more common. One is therefore apt to confuse the relative importance of stone and wattle buildings during past ages.

The following account by Arthur Mitchell, in ' Vacation Notes ' is, by the kind permission of the Society of Antiquaries of Scotland, reprinted from their Proceedings, 1875, x, 609–11.

' St. Wallach is said to have lived in a poor little house woven together of reeds and wattles, and this seems to have been reckoned a hardship and a mark of humility. We cannot, however, safely infer from this that the people among whom he laboured, and who were neither self-denying nor humble, lived in houses of a different and more comfortable character. Indeed, it seems surprising to find so much made of the saint's wattled dwelling, since houses constructed more or less completely of *wattle and daub* still exist among us ; and I know that they can be tolerably comfortable. At Inverie, in the parish of Glenelg, I once[1] slept in a house the partitions of which were of wattling plastered with clay ; and I find an entry in my journal (16th October 1866), in which I say that in Kintail, Glengarry, Glenmoriston, and Lochaber, " wattled partitions plastered with clay are frequent—outside walls too, but these generally for sheds or byres ". Another entry occurs, 17 September 1864, very much to the same effect. Captain Thomas tells me, from personal observation, that wattling was till lately, and probably still is [1875], common in some parts of the Highlands. Nor was it confined to the dwellings of the poor. In Duthil, for instance, the old house of Inverladnan, in which Prince Charles passed a night, had creel partitions ; so had the house of Shillochan in the same

[1] In 1859, or 1860, Mr W. F. Skene, when reading these notes in proof, kindly furnished me with the additional fact that the well-known Macdonell of Glengarry built one of these wattled houses at Inverie, and occupied it as a summer residence for many years. Mr Skene adds that he has often slept in it.

parish.[2] At Stronmilchain, in the parish of Glenurchy and Inishail, M'Gregor of Glenstrae had a house built of wattles, said to have been surrounded by a moat, and accessible only by a drawbridge[3]; and about the end of the eighteenth century, the second son of Cluny, afterwards the Chief, brought home his bride, a daughter of Lochiel, to a wattled house at Nuid, near Kingussie.[4]

'Walker, again, writing between 1760 and 1786, says, "When wood is at hand they erect what is called creel houses. These are formed of wooden posts, interlaced with branches of trees, like wicker-work, and covered on the outside with turf".[5]

'When we hear of persons living in creel houses, it would be entirely wrong to conclude that they are either half-barbarous or peculiarly wretched. The fact may be nothing more than an outcome of circumstances, and no mark at all of inferiority or misery. The early settlers in Australia were persons who had been born and brought up in stone houses, in which not a few of them had lived luxuriously, yet we read that their houses in Melbourne "were most primitive in form and construction", and "were chiefly limited to one storey, and usually built of wattle-stems interlaced, and plastered with mud, known as wattle and daub".[6] Mr Robert Tennent tells me that he long lived in such a house, of which he has shown me a photograph, and in which he enjoyed no little happiness and comfort.

'Wattle-built churches[7] were common, and have received notice at the hands of various writers[8]; but in the "Martyrology of Donegal"[9]

[2] I state this on the authority of Mr David F. Ross. It was in the house of Shillochan that a remarkable piece of Scotch carving on fir was lately discovered. See *Inverness Courier* of 24 September and 15 October 1874.

[3] *New Stat. Acct.* vol. VII, p. 97.

[4] *Book of Deer*, by Dr Stuart, preface, p. CLI.

[5] Walker's *Econom. Hist. of the Hebrides and Highlands*. Edin. 1806, vol. I, 95.

[6] *Glimpses of Life in Victoria*. By a Resident. Edin. 1872, p. 10. See also *Voyages to the East Indies*, by John Splinter Stavorinus, Lond. 1798, vol. I, p. 57.

[7] An oratory built of wood, wattling, or clay was said to be built 'after the manner of the Scots', and one of stone 'after the Roman manner'. The wooden oratory which King Edwin erected at York was enclosed in the stone church which he afterwards built there; and William of Malmesbury tells us that the 'old church of wattled work'—St. Mary of Glastonbury—was, at the end of the sixth century, 'covered with a coating of boards' by order of Paulinus, Bishop of York. (See Babington, *Arch. Camb.* 1857, vol. III, p. 147).

[8] Dr Stuart, *Book of Deer*, pp. CXLIX to CLVII; R. Perrott in the *Arch. Camb.* 1857, vol. III, p. 147; and Reeve's (*sic*, for Reeves's] *Adamnan*, pp. 106, 114 and 177.

[9] op. cit. p. 177–9.

there is an interesting reference to one, for which, as for very many other references, I am indebted to Mr Anderson. We are there told that Mochaoi or Caolan, Abbot of n'Aondrium [*sic*, for Nendrum], in Uladh, went with seven score young men to cut wattles to make a church, and that he was himself cutting timber like the rest when a bird, " more beautiful than the birds of the world ", spoke with him from a black thorn close by, and continued speaking for 300 years, while the saint slept " a sleep without decay of the body "—his bundle of wattles lying the while by his side, like himself keeping fresh and without decay. Machaoi is said to have been alive in A.D. 496. St. Patrick gave him a crozier, called from the legend of the Wattles the Baculus Volans, or Winged Crozier '.

DISCOVERIES IN ROME (PLATES I–II)

Some of the most important discoveries of recent years in Rome have resulted from excavations in and near the Palace of the Cancelleria. It was in this part of the Campus Martius that the consul Aulus Hirtius received state burial after the siege of Mutina in 44 B.C. ; and now a part of his funerary precinct has been found, with two boundary-stones bearing his name. The other objects from these excavations were not *in situ*, hence their interpretation is more open to conjecture. The period of Tiberius or shortly later is represented by a long slab of marble containing most of one face, and the adjoining portion of another, of the frieze of what was presumably a square or rectangular monument ; it contains many figures, some of which are represented as holding figurines of Lares. Six other slabs, on a larger scale, in fact over life-size, clearly belonged to a monument of about the year 93 of our era, as they include the representation of the triumphal return of Vespasian and a similar appearance of Domitian. Their surface is almost perfectly preserved ; the structure to which they belonged cannot have stood very long before it was dismantled and its sculptured details placed, face inwards, leaning against a wall ; it is probable that a flood of the Tiber buried them with alluvial deposit in that position, thus protecting them from deterioration and discovery until the year 1938. Both the Tiberian and the Domitianic slabs are sure to claim a prominent place in future discussions of Roman art : the former are distinctly in the tradition of the long sides of the balustrade of the Ara Pacis Augustae, while the latter anticipate the stylistic qualities of Trajan's Arch at Beneventum. The generosity of Dr Filippo Magi of the Vatican Museum enables us to show, in PLATES I and II, a general

EARLY IMPERIAL FRIEZE FROM THE CANCELLERIA, ROME (*see* p. 344)
Courtesy of Dr Filippo Magi

PLATE II

DETAIL FROM THE FRIEZE REPRESENTED ON PLATE I

Courtesy of Dr Filippo Magi

PLATE III

CURVED HORN SICKLE FROM KARANOVO, BULGARIA (*see* p. 348)

PLATE IV

PIRIFORM PITCHER FOUND AT KARANOVO (*see* p. 349)
Height 18.7 cm.; diameter 13.1; mouth 7.5

FIG. 4. SUBMERGED BOAT, YORKSHIRE (*see* p. 349)

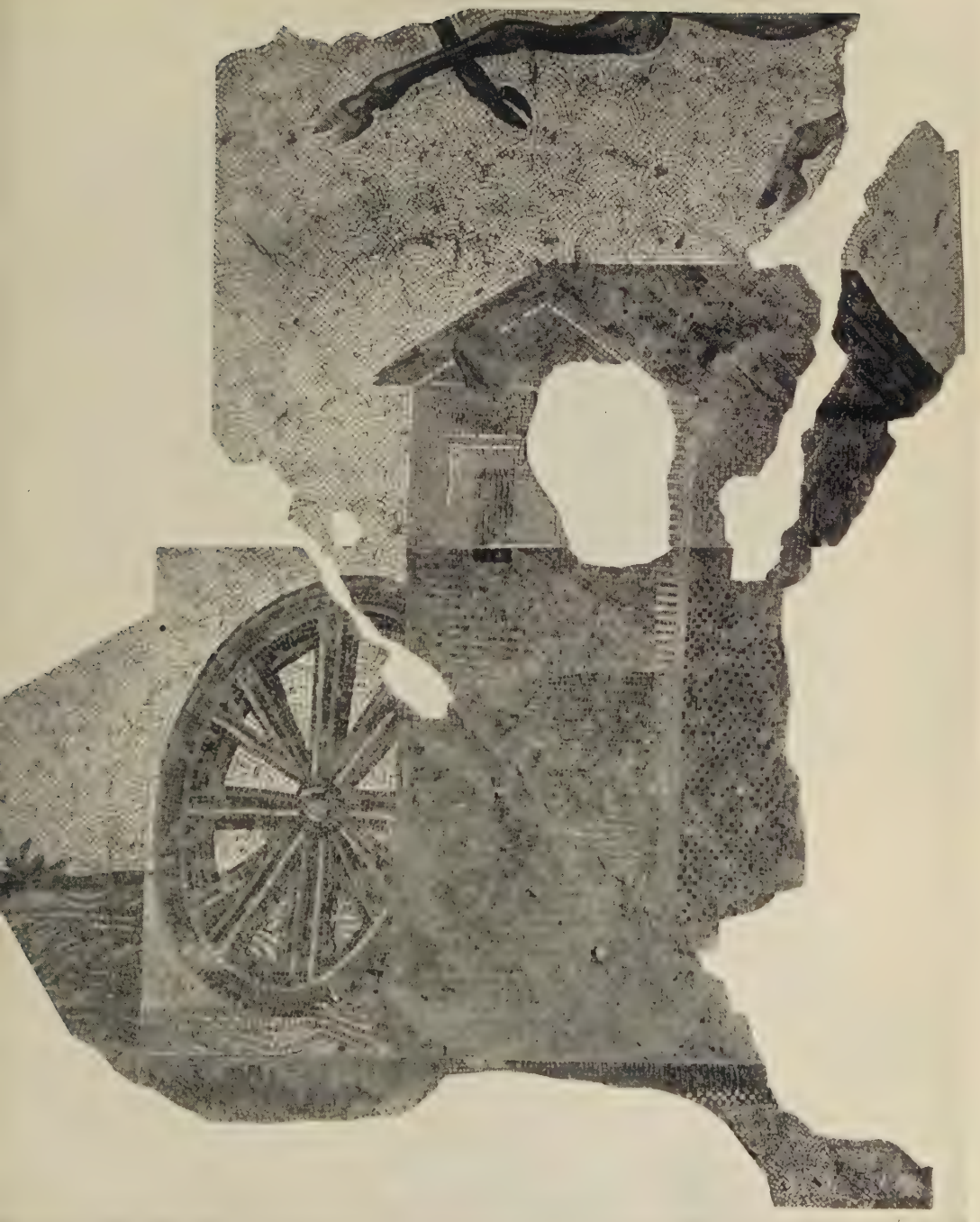
Site of boat at low tide, looking east, and showing western end after excavation. Six feet of the centre plank are shown. The eastern end is marked by a stake in background

FIG. 5. SUBMERGED BOAT, YORKSHIRE

Five planks in place with battens and ties (each arm of the rule is one foot) In the foreground is the batten mentioned on page 352

FIG. 6. SUBMERGED BOAT, YORKSHIRE (*see* p. 352)
Mast step (?) on centre plank near middle of the boat

PLATE VII



MOSAIC (5TH CENTURY OF A ROMAN WATER-WHEEL FROM THE GREAT PALACE OF BYZANTIUM (*see* p. 354)

PLATE VIII

RAS SHAMRA-UGARIT: POSTERN GATE AT THE FOOT OF THE STONE GLACIS (*see* p. 358)
Ph. Claude F. A. Schaeffer

view and a detail of the earlier frieze. All these monuments (except for one of the Domitianic slabs, which was found outside the territory of the Cancelleria and hence belongs to the City of Rome and has been taken to the Antiquarium of the Caelian), together with the epigraphical and religious remains from late antiquity which were discovered in the same excavations, and which considerations of space prevent us from describing, have been taken to the Vatican where they will occupy a place of distinction ; Dr Magi's treatment of the friezes when it appears in print will arouse the greatest interest.

We wish to thank Dr A. W. Van Buren, of the American Academy in Rome, for his good offices in this matter and for the above description.

KARANOVO, BULGARIA (PLATES III–IV)

North of the village of Karanovo, in Nova Zagora, southern Bulgaria, is one of the largest mounds in the country, some 12 m. high with a diameter of 110 by 180 m. For years the local villagers have used this mound as a source for fertile earth, and have found various small objects now deposited in Nova Zagora Museum.

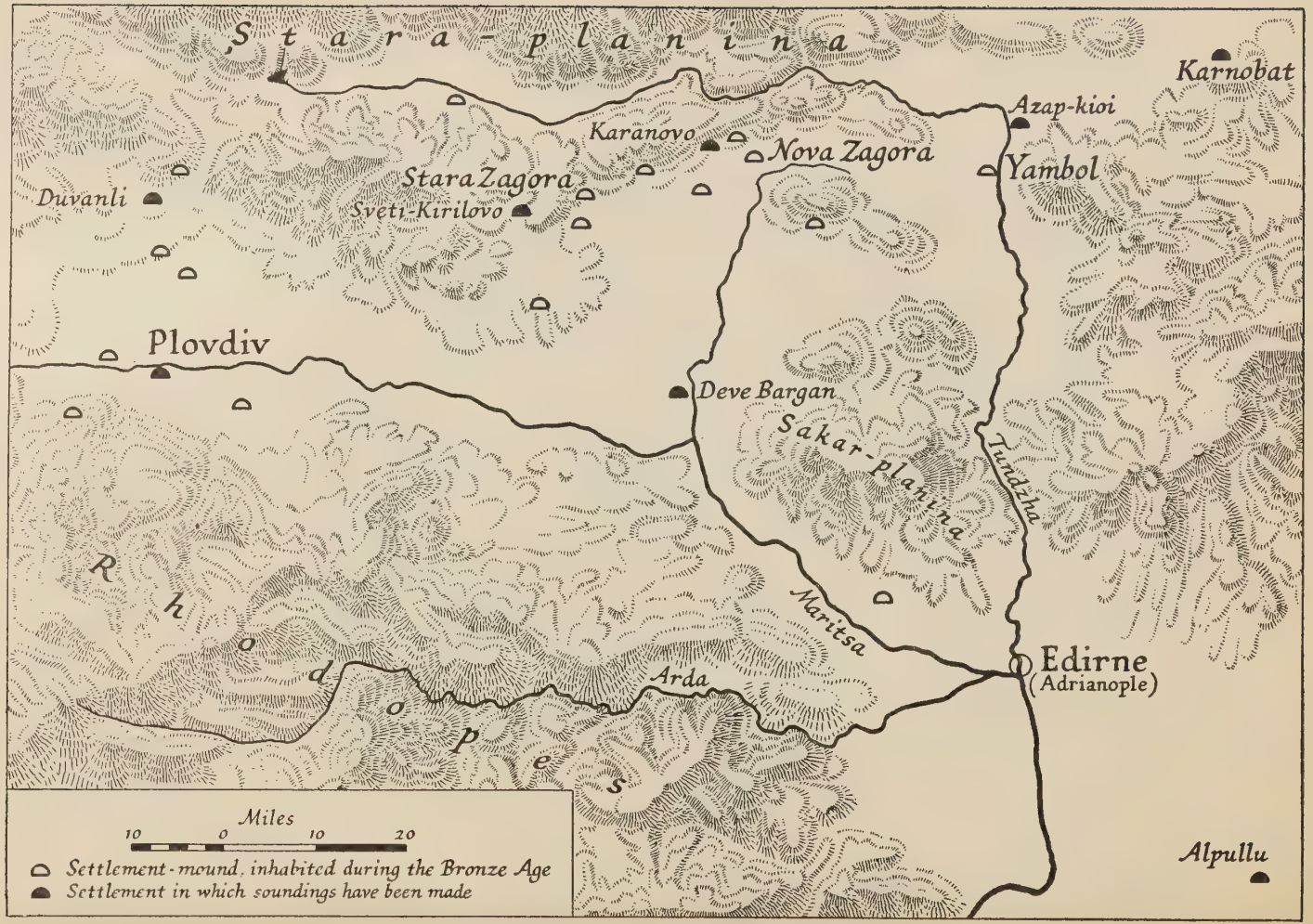
Recently the National Museum of Sofia conducted a trial excavation in the exposed eastern and southern slopes of this mound. It revealed the first definable stratigraphical superposition in Bulgaria of the Chalcolithic and Bronze Ages. Sherds, several complete pots, traces of huts and wall-plaster, stone and bone tools, animal bones and grain were found in both levels.

Remains of three rows of reinforced walls indicate that this settlement was a fortified village in its prehistoric past.[1] These walls were made of earth revetted with pitted limestone slabs set close together without mortar, and inclined inwards. The stone is of local origin, derived probably from Sredna Gora, 1½ km. north of Karanovo. Three fragments of a wall 1.10 m. wide were found on the south side of the mound, varying from 0.30 to 0.90 m. in height,

Two strata were observed at Karanovo, the lower level, 4 m. deep, yielding objects characteristic of the ' Bulgarian Tell Culture '. The upper stratum, whose 8 m. depth will be capable of further subdivision, produced pottery similar to the upper level at Sveti Kirilovo[2] and Asparukhovo (unpublished) near Stara Zagora, and at Zagortzi (unpublished) near Nova Zagora. This later pottery from the second level

[1] Tell Metchkur (B.C.H. 1906, p. 406 sq.) and Balbunar (*Izvestiia Bulg. Arch. Inst.* IV, 252–4) were also fortified.

[2] P.Z. 1914, pp. 64–75.



BRONZE AGE SETTLEMENTS, SOUTH BULGARIA

is also similar to material found at Azap Kioi near Yambol,[3] Shopova Mound (unpublished) near Stara Zagora, Graf Ignatief near Plovdiv (unpublished), and Duvanli,[4] also near Plovdiv.

The chief differences between the two levels at Karanovo are found in the pottery. The remains of huts, millstones, flint and other stone artifacts, and the bone material, did not differ radically in the two strata.

In one section of the mound the remains of many huts were observed. They were quadrangular in form, and built on a pole frame, the walls made of clay mixed with straw, applied to wattling. The wall-plaster was found to have been fired to make it more firm. The huts usually contained a hearth in one corner, and a mill-stone in the other.

There was evidence that the huts had been destroyed by fire, and that, before subsequent rebuilding on the site, objects had been removed. Over the fallen wall-plaster of a ruined hut was laid new earth, on which the later huts were built.

Objects were found in only three of the huts. From one in the upper stratum on the south side of the mound came two complete vessels and the fragments of another. In the lower level on the east side the ruins of a hut were found to contain three vessels. In one of these pots three stone celts and a pile of mussel shells had been put, and in another a lump of white material for use in ' incrusting ' decoration. A hut in the upper level on the east side yielded two vessels, sherds and clay sling-bolts.

The stone objects from Karanovo were varied, including mill-stones of typical Chalcolithic (Bulgarian) type,[5] and celts, in size and shape like those from other Bulgarian mounds. A few fragments of perforated stone hammers and axes came from both levels ; and a core from the perforation of a stone axe, was also found, indicating that the weapon was drilled on the spot. This core is doubly interesting, for the stone is not a local Bulgarian variety, but is probably derived from Anatolia. Several spheroid stone ' war-balls ' were also found.

In the mounds of southern Bulgaria flint objects are considerably rarer than in those from north of the Balkan range, depending perhaps on the presence of natural flint outcrops, which are scarce in the south. But there are many in the north, in the Novi-Pazar, Shumen, Nikopol

[3] Godishnik Plovdiv 1930, 171 ff.

[4] B. Filov, Nadgrobni moghili pri Duvanli 1934, 26 ff.

[5] V. Mikov, Stations et trouvailles préhistoriques en Bulgarie, p. 54.

and Belogradchik regions. At Karanovo flint scrapers and knives, comparable to those from similar local mounds, were found.

A considerable quantity of bone and horn objects was found in both levels of Karanovo mound, including spear and arrow heads, the usual awls, chisels, and a perforated horn adze. These are like similar objects in previously published mounds. From the upper level at Karanovo came two bone pins of rather interesting type, with expanding, flattened, triangular heads.

But the most interesting object of horn was a sickle, found in the upper part of the lower cultural stratum. This curved horn, which is thicker at one end than at the other, is 31 cm. long, and 3.5 cm. thick at the handle end. Along the upper side, throughout almost its whole length, a deep groove had been made, in which were set a series of small jagged flints, four of which were found in position (3 to 4 cm. long, 1.1 to 1.4 cm. wide), set close together. The outer edges of the teeth were polished by use in cutting corn (PLATE III).

Similar flint-sickle teeth have been found in other Bulgarian mounds, and a few from Karanovo are deposited in the Nova Zagora Museum. Another, perforated at one end, was found at Malkata Peshtera[6] near Tirnovo.

The pottery from the lower level (A) at Karanovo belongs with few exceptions to the Chalcolithic period. It is a handmade ware, the paste well cleaned. Its exterior colour is reddish, though it sometimes varies to dark black. The forms and decoration are characteristic Bulgarian Tell Culture types. In the upper part of the lower level were found high hollow bases, with and without perforations.

The decoration of this A ware was varied, including graphite painting and sherds ornamented with white painted lines, the lines easily being removed, and probably painted after firing. The graphite painted pottery occurred throughout in all depths of this level, linear decoration and wolf's teeth, etc., on red or on blackish ground, the patterns positive and negative (reserved). The white painted ware was found only in the upper part of the level, more frequently on vessels with high hollow base, and occurs only on red ware. The sherds illustrated are in soft white crumbly paint on polished red-brown ground.

Relief decoration (plastic ridges, etc.), finger-marked and incised designs were constant features throughout the levels.

[6] R. Popov, ' Belyakovskoto Plato ', p. 13.

While the Chalcolithic pottery of Bulgaria is noted for its variety of ornament, the pottery of the subsequent period is almost without decoration. Most frequent were shallow incisions on the everted rims of the shallow bowls with four feet. Small zigzags, single or double lines, were incised on the walls and sometimes on the handles of a few of the vessels. Relief decoration was infrequent.

From all depths of the upper level came cylindrical handle fragments, the upper ends of which were furnished with a cylindrical projection, or wide and flat, or conic lug. All these handles were from large or small cylindrical-to-piriform pots (PLATE IV).

Bones of deer, gazelle, cattle, sheep, pig, etc., were found in both levels. In the upper level bones, teeth, and a hoof of the horse (*Equus caballus*). Until now no Chalcolithic settlement, either in Bulgaria or surrounding countries, has yielded horse-bones. But by the Middle Bronze Age the horse is in the group of domesticated animals. Some bones and teeth of horse were also found at Azap Kioi.

The prehistoric occupants of the Karanovo mound definitely practised agriculture. Wheat, vetch and varieties of peas were found in the lower level. More than ten bushels of carbonized wheat-ears, show that grain was stored for subsequent usage after harvest. In the upper level were some carbonized wheat and varieties of small peas.

The Karanovo mound is important in the prehistory of Bulgaria for the clearly observable presence of two cultural horizons, the lower four metres containing remains of the typical Bulgarian Tell Culture material. The deep upper level B, analogous in some respects to the upper part of Sveti Kirilovo, shows similarities with the Bereketska Mound, with Zagortzi, Azap Kioi, Duvanli, and Shopova Mound, all of which are to be dated within the Bronze Age.

VASIL MIKOV,
National Museum, Sofia.

SUBMERGED BOAT AT NORTH FERRIBY (PLATES V–VI)

In the summer of 1937 we found between tide-marks on the Humber shore east of North Ferriby the projecting ends of three planks. A little rather destructive cleaning was done and the planks traced by prodding with a walking stick. They were found to extend for over forty feet at a depth of about three feet. It was now fairly obvious that the structure was the remains of a boat of some sort. Attempts were made to discover the method of construction and at first it was thought

that the planks were pegged together. Later what seemed to be the bases of worn perforated cleats suggested that the boat was very early, and at one time we thought of a Viking date, but this was finally ruled out and we decided on further excavation. Three sections were uncovered, cleaned, photographed and filled in again. A large number of photographs were taken and careful measurements made. Early this year we removed about 6 feet of the eastern end, to experiment with methods of preservation.

It is now possible to give at least a preliminary description of the boat, which probably presents most of the essential details. We hope in time to remove the boat entirely, but the difficulties are great; the maximum working time between tides is about five hours, the boat is half a mile from any road and the sticky mud makes digging unpleasant. Probably the boat will have to be removed in short sections and may suffer a certain amount of damage in the process.

The boat lies obliquely to the line of the Humber, about 20 yards from low water mark and half a mile east of the end of the road at North Ferriby. It is buried in a stiff grey marl, and rests on a flinty gravel at the top of the boulder clay. At this spot the Ferriby Peat Bed and its underlying Green Seatearth had been eroded before the Grey Marl was laid down. The diagrammatic stratigraphical section of the site is shown in FIG. 1.

The boat is made of oak planks up to four inches thick. They are set edge to edge and the seams caulked with the moss *Neckera complanata* Huebr. Over the seams so caulked are thin battens of oak about half an inch thick and up to $2\frac{1}{2}$ inches wide. At intervals of about a foot are pairs of holes, one at each side, through which are threaded withes or ties of twisted yew, sewing the planks and batten together. On the inner plank at each join the tie does not go completely through the plank but comes out in the furrow at the side. (FIG. 2, PLATES V, VI).

There is a central dubbed out plank 43 feet long and from 20 to 24 inches wide. Two planks are preserved on each side, and from the yew-ties still in place at the edges of the outer planks it is clear that there has been at least one more plank on each side. At present the planks lie flat, but originally they must have looked in cross-section something like FIG. 3; and the western end of one plank still rises at an angle of thirty degrees or more. So far we have no evidence to show how the planks were supported in the curved position, but it was probably by cross-thwarts attached to the outermost plank.

So far as can be seen the centre plank is hewn from one trunk. The

various projections on it are of a piece with the rest of the plank, and its western end is carved out. These projections are from east to west :

(1) Two groups of four rectangular knobs, 3 inches by 4 by 1, the two groups about a foot apart. Parallel to the western group there are two similar knobs on the next plank to the south.

FIG. 1. STRATIGRAPHICAL SECTION OF SITE OF BOAT
1, Grey Marl 2, Peat 3, Seatearth 4, Boulder clay
(Vertical scale exaggerated and diagrammatic)

FIG. 2. METHOD OF JOINING THE PLANKS
Dotted line = Yew tie X = Batten A = Centre plank B = Side plank

FIG. 3. PROBABLE ASPECT OF ORIGINAL CROSS-SECTION OF EASTERN HALF OF THE BOAT

(2) To the west of (1) is a double ridge 8 inches long, 10 broad and 4 high lying in the centre of the plank. The area between the two parts of the ridge is not hollowed out right to the level of the plank.

(3) At the east end of section II (PLATE V) there is a double knob in the centre of the plank and in the middle of II a group of four irregular knobs, of which the two southern ones are L-shaped.

(4) Just west of this the centre plank swells up to a height of four inches. Possibly the swelling is a block of wood repairing a hole, tied down with a cross batten which passes under the longitudinal ones (FIG. 5). The hole, if there is one, may have been due to a branch leaving the main trunk at this spot.

(5) In the middle of III there are two large projections, in line fore and aft, one about 4 inches square, one roughly rectangular, 18 by 4 inches. They both taper upwards and are about 5 inches high. (PLATE VI).

(6) Three feet from the west end of the boat there is a low ridge across the centre plank, like those on the floor of the Brigg dugout in the Hull Museum. That something was fixed to it is shown by a yew-tie still in position across its northern end. (PLATE VI).

The only suggested explanation for the various knobs is that they served to fix supports for decking or ribs to preserve the curvature of the sides.

Several joins in the battens were noticed. The overlapping ends are carefully smoothed and rounded off. At the east end of III (FIG. 6) one line of battens ends. Apparently the plank nearest the middle on the south side was not broad enough for the eastern half of its length and another plank had to be inserted here. The method of joining must be in doubt until further excavation. The same sort of thing probably happened just west of II (PLATE V) on the northern side.

Few objects were found in or near the boat. There were loose pieces of wood, oak and alder (*Alnus glutinosa L.*). Apparently reeds of some sort had been rammed into the surface of the planks. A few halves of acorns and hazel nuts, two shells of small crabs and a lump of white flint was all else that we found. About 50 yards away we recovered most of a paddle, from apparently the same layer of grey marl as the boat. It is of pine with a cylindrical handle, and blade flat one side and gently rounded the other. Its present dimensions are: length 40 inches, maximum breadth of blade 6 inches, diameter of handle $1\frac{3}{4}$ inches: the blade was originally about 7 inches wide.

Thus there are no external indications of date and we must consider stratigraphical and internal evidence. The former is not much use. The Green Seatearth below the Peat here is 'perhaps Maglemose'; some tufaceous marl and sandy peat from a buried stream bed in the peat is dated to before 1800 B.C. by A. S. Kennard on the evidence of about forty species of snails that it contains.[1] High up on the strand,

[1] C. W. and E. V. Wright, 'Holocene Deposits at N. Ferriby', *Naturalist*, Sept. 1933, pp. 210–12.

where the peat is only 6 inches thick, we found a flint implement *in situ*, according to M. Burkitt probably of Early Bronze Age. Thus any post-peat object at Ferriby is probably later than Early Bronze Age. The grey marl in which the boat is buried is merely an early Humber warp and an apparently continuous bed of it may well be of different dates at different spots. Stratigraphy only suggests that the boat is later than Early Bronze Age, which was fairly obvious already.

Internal evidence does not carry us much further. The boat is presumably of later type than dugout canoes and earlier in *type* than plank-built boats of Roman[2] or Veneti[3] build. But it may be native built and Roman in date. It cannot be post-Viking, since the Humber keel of today suggests continuity with Viking vessels on the Humber and no post-Viking vessels had the planks sewn together. There is an attractive, but as far as we can see unsupported, conjecture that it belonged to the local Parisi, the tribe that inhabited east Yorkshire when the Romans advanced north. There was a settlement of these people on Red Cliff about a mile to the west of this site. A quantity of imported pottery of Tiberian and Claudian date, before the Roman advance to Yorkshire, has been found there by us, and it seems likely that there was for a time considerable trade with Gaul.[4] However the attribution of the Ferriby boat to the Parisi is pure conjecture and at present we can hope for nothing better.

The closest parallels are the large 'sewn' boats of Ceylon and Oceania, and one suggested to us by Mr Hornell. In *Man*, 1928, 1, he described and figured some boats possibly derived from Indonesian culture, in use on Lake Victoria Nyanza in East Africa. They are made of large strakes on a broad dugout base, the seams are caulked with plantain fibre, covered with a strip of wood and sewn with twine passing through opposed holes in edges of the planks, as in the Ferriby boat. Except that the holes are close together and the twine continuous in the African boats the resemblance in these methods of construction is very close. Whether it is more than a coincidence we would not like to say.

We are indebted to Mr Hornell for the following note :

'From the absence of any trace of the presence of frames (ribs), I would conclude that this boat represents a crude and very early

[2] 'Ship of Roman period from site of New County Hall', L.C.C. 1912.

[3] Caesar, Bell. Gall. III. 13.

[4] P. Corder and T. D. Pryce, 'Belgic and other early pottery found at N. Ferriby, Yorks'. *Antiquaries Journal*, July 1938, XVIII, 262–77.

development from the dugout canoe type having transverse strengthening ridges on the floor ; it appears to be antecedent typologically to the Scandinavian prototypes of the Viking type of clinker-built boat in which frames are present, tied to lugs or cleats projecting from the inner surface of the planking. This view is strengthened when we compare the system of sewing the planks together in the Ferriby boat with that in the early pre-Viking clinker type—in the former by means of twisted yew ties passed through large holes spaced widely apart ; in the latter with fibre twine sewing passed through smaller and more numerous holes.

The strangest feature of the craft is the unique manner in which the edges of the planks are fitted together and to the edges of the bottom dugout base-plank. In this system the lower edge of each side-plank is cut to a rounded bevel angle in order to fit securely into a v-shaped groove in the upper edge of the one to which it is joined. I know of no parallel to this, for the only craft in which the planks are not butted flat edge to edge (carvel build), or overlapped (clinker build) is the zigzag tongue and groove system seen in boats built on the Gujarati coast of India (see J. Hornell, *The Tongue and Groove Seam of the Gujarati boatbuilders*, *Mariners' Mirror*, pp. 310–12, 1930) '.

Our thanks are due to Messrs. M. W. Barley, H. H. Brindley, P. Corder, A. L. Congreve, J. Hornell, W. MacDowell, T. Sheppard and the authorities at Kew for suggestions and help in many ways.

C. W. and E. V. Wright.

BYZANTINE WATER-MILL (plate vii)

The curious building illustrated is a detail of the mosaic which has been uncovered by the Walker Trust (St. Andrews University) excavations in the Great Palace of Byzantium. It is of some importance as being the only example of a water-mill in Roman art. Considering its late date—the mosaic is dated to the early fifth century—this is at first sight surprising, since we know that an ὑδραλέτης was among the possessions of Mithridates of Pontus which fell into Pompeius' hands in 65 B.C. (Strabo, XII 3.30). But in spite of the age of this, the first water-mill of which we know, references to water-mills are rare in Roman literature. Beyond the well-known epigram of Antipater (Anth. Pal. IX, 418, 3–6), a line of Lucretius (v. 516), and a remark of Pliny's ' Major pars Italiae nudo utitur pilo, rotis etiam, quas aqua verset, obiter et mola ' (H.N. XVIII, 23), and excepting, of course, the

description of Vitruvius' mill (x, 5, 2), the water-mill is not heard of until the late Roman period. This absence of literary record is paralleled by an almost total lack of evidence of actual mills. Except for the mill-wheel from Venafrum, in Naples, and the evidence of the mills by the Janiculum[1] and on the Wall[2] none are known.

From the fourth century onwards, however, evidence is more plentiful. Palladius (R.R. I, 41) recommends the use of the outflow of water from the public baths, a remark which implies a fairly wide distribution of mills, at least in the towns. Regulations for mills which must be visualized as being outside the towns are given in the Salic and Visigothic Laws,[3] while Digest. XXXIX, 2.24 may refer to mills as well as other buildings. Other references to water-mills are made by edicts of Honorius and Arcadius, A.D. 398 (C. Theod. XIV, 15.4), and of Zeno (undated ; C. Just. XI, 43.10), and by Prudentius (contra Symm. II, 949–50)—all these referring to the mills by the Janiculum ; and by Ausonius (Mosella 361–4), Venantius Fortunatus (Misc. III, 12, 37–8),[4] and Procopius (B.G. I, 19). A fourth-fifth century epitaph of a *μανγαναρείος ὑδραλέτα* is recorded from Sardis.[5]

This literary evidence can fortunately be supplemented by the mill found in the American excavations in the Agora at Athens, and dated to the reign of Leo I (457–74).[6] Enough of this remains to allow a reconstruction proving that it is based on the Vitruvian plan ; the only innovation is that it is overshot. Vitruvius' mill was clearly undershot, as is the mill on this mosaic, and as were the only mills referred to in the sources about which the question can be settled—Ausonius' mill in the Moselle and Belisarius' floating mills in the Tiber. The overshot wheel is a development from the undershot, canalizing and increasing the force of the water. The literary sources show the extension of the water-mill in the later Empire ; and until further evidence is obtained, the invention of the overshot wheel may be attributed to the later Empire too[7]—an argument in favour of more inventive ability than is generally allowed to have existed in that period.

[1] Van Buren and Stevens, *Memoirs of the American Academy in Rome*, I, p. 59 f and XI, p. 72.

[2] *Proc. Soc. Ant. Newcastle-upon-Tyne*, 3rd series, IV, 167.

[3] Bennett and Elton, *History of Corn Milling*, II, 42, and Lex Salica, XXIX.

[4] For this and other references I am indebted to Mr C. E. Stevens.

[5] Buckler and Robinson, *Sardis* VII, 138, no. 169.

[6] A. W. Parsons, ' A Roman Water-mill in the Athenian Agora ', *Hesperia* V, 70 ff.

[7] As against Parsons' theory that Antipater was referring to an overshot wheel, see Bennett and Elton, *op. cit.*, II, chap. I.

The curious and unfortunate fact about this picture lies in the inexplicable position of the wheel. If it is meant to be in the same plane as the wall on which it casts a shadow, the whole wheel should be visible ; if in the same plane as the wall we do not see, it is in perspective from the wrong side. In either case, it is scarcely reconcilable with Vitruvius' description. The picture is an interesting and important corroboration of the evidence we already possess ; but about the design of the mill it can tell us little. G. BRETT.

THE PLOUGH AND TERRACING

In the article on ' The Plough and the Origin of Strip-Lynchets ' by Dr E. Cecil Curwen in the March number of ANTIQUITY, there is a reference to the possibility of ground sloping at an angle of 20° being ploughed parallel to the contours without artificial terracing. There is on my farm here [Cressage, Shropshire], some pasture land which has been ploughed parallel to the contours, where the angle of the slope is in places 20° or even slightly steeper. This is almost certainly early 19th century work done with a swing plough.

A slope at an angle of 20° appears to be about the limit at which ground can be ploughed without terracing ; it is much too steep to be ploughed uphill and it is probably equally difficult to plough it downhill. Most of the land in this county is now, and appears always to have been, ploughed at right angles to the contours, always excepting some very steep slopes (now pasture) which have been ploughed parallel with them.

True strip lynchets are very rare in Shropshire, I know of only one example and that a poor one. It is perhaps significant that this solitary example is situated on the Wenlock Limestone. W. J. SLACK.

RAS SHAMRA-UGARIT (PLATE VIII)

Monsieur C. F. A. Schaeffer very kindly sends us the following brief summary of the results achieved during the 10th and 11th campaigns in the winter seasons of 1938–39 at the site of Ras Shamra, the capital of the ancient kingdom of Ugarit in Northern Syria.[1]

The Expedition discovered a new quarter of the town dating back to the middle of the second millennium B.C. The houses were large and each of them had a burial vault in the basement : both houses and

[1] Résumé of a lecture to the Academie des Inscriptions et Belles Lettres, Paris, 16 June 1939. We wish to thank Mr M. E. L. Mallowan for his help in this matter, especially for translating the note.

tombs produced many objects, including clay and bronze vessels, weapons, seals, amulets and a variety of ornaments. Among the finds was a painted clay cup with walls of egg-shell thickness, a specimen of the celebrated Kamares ware, manufactured in Crete and imported to Ugarit. This discovery enables us to lower the date assigned to the end of Middle Minoan II by a century.

From a family vault of the 14th century B.C. we recovered the skeleton of an adult whose vertebral column had been pierced by a bronze arrow, which was still lodged in the centre of the medullary canal, inflicting a wound which according to Dr Jarry must have brought about almost instantaneous death. The arrow had entered the shoulder just as the man was about to attack an enemy who must either have been in a chariot, or else on some platform, at a level higher than that at which he himself was standing. This discovery was part of a chain of evidence proving the number of battles in which the city had been involved. Ras Shamra-Ugarit was not only a commercial and intellectual centre, but as the new discoveries now show, a perfectly equipped military stronghold, occupying a strategic position of great importance in Northern Syria.

In the official quarter of Ugarit the Expedition uncovered the royal stables, as well as the residency of the military governor, who was a son of the King of Beyrouth. Here we discovered a large clay tablet inscribed in the cuneiform character : this tablet is an inventory of the armaments kept in the arsenals and allotted to the soldiers who included bowmen and slingers. Near by, the excavations revealed another important building which was a part of the Royal Palace and contained the economic, diplomatic and private archives of the kings of Ugarit. These inscriptions sometimes make use of the celebrated alphabetic script invented by the scribes of Ugarit. They also make use of Babylonian, and sometimes the two different systems occur together. The new texts contain records of the taxes which the various cities in the kingdom had to pay and the number of soldiers they had to supply. There are several letters from foreign rulers, addressed to the king or the queen of Ugarit. One of these letters comes from the great Hittite king Shubiliuluma, a contemporary and enemy of the Pharaohs Amenophis III and IV, who were competing with him for predominance in this vital sphere of influence, a focal point on the trade routes of the ancient East.

Not far from the building containing the archives we discovered the Palace of Ugarit : the use of copper column bases overlaid with

silver bear witness to its former richness. These bases recall the copper columns placed by Solomon in front of the sanctuary of his temple in Jerusalem and referred to in the Bible.

The Ugarit Palace was protected by an imposing fortified bulwark, the nucleus of which was uncovered in the course of the same campaign. It consists of a strong square tower, 14 metres in width, flanked by enormous glacis, which are entirely revetted by heavy stones. A vaulted subterranean corridor running from the interior of the fort to the foot of the glacis, and debouching on a formidable postern gate, enabled the defenders to rush the enemy in case of attack. This work is a revelation to us of the remarkable achievements of the military architects of Northern Syria as early as the middle of the second millennium B.C. Similar fortifications in Palestine, built on a more modest scale, may possibly have derived their inspiration from this source. (PLATE VIII).

Moreover at that time the port of Ugarit was one of the trading centres of Egeo-Mycenaean colonists in the East, and this work is therefore also an indication of the influence of Mycenaean military architecture, of which the principal monument is the fort uncovered by Schliemann at Hissarlik, generally reputed to be the Homeric Troy. Ugarit suffered the invasions of the Peoples of the Sea, who burnt and sacked it at the same time that they reduced Priam's city to ashes.

The Expedition also discovered a number of ritual foundations which contained large jars with perforated bases, deposited or buried vertically in the ground, intended to receive libations of water and other liquids. This ritual was designed both to secure a sufficient rainfall—this being essential to prosperity in a country with a scorching sun—and to refresh the dead who were buried in the vaults of the neighbourhood, sometimes in direct contact with these foundations.

The jars from Ras Shamra with holes in their bases explain[2] the meaning of the curious myth of the Danaids, which our own contemporaries still refer to without understanding its import, any more than the Greeks themselves understood it. According to the well known legend, the 50 daughters of Danaos who had been ordered by their father to kill their husbands on the night of their wedding feast, were condemned by the gods to pour water ceaselessly into a jar with no base to it. The significance of this very ancient rite had been forgotten by the Greeks and was interpreted by the mythographers as a

[2] See our *Cuneiform Texts of Ras Shamra-Ugarit*, British Academy, London, 1939, pp. 46–56.

refined torture imposed by the gods upon these murderers. But actually, as the Ras Shamra jars with their pierced bases show, this was merely the ritual fulfilment of a funerary ceremony intended to secure eternal peace for the murdered husbands. If on the other hand we relate these jars to a fertility ritual, the result is just as helpful in supplying the key to the problem of the Danaids. For according to the oldest tradition of the story as related by Hesiod in the 8th century B.C., the Danaids were regarded as beneficent heroines who had brought water and fertility to the Argolid, a dried up country. In the earliest representations then, the Danaids appeared in the guise of water carriers filling one of these jars intended to produce fertility by the medium of appropriate libations, like those revealed by the discoveries at Ras Shamra. Indeed, on the earliest monuments representing the Danaids, the water running through the jar is not depicted by the artist as running out at its foot, for in accordance with the ritual it had to be dispersed deep down in the ground. This detail was only added under the influence of the legend on late monuments, at a time when the meaning of the task of the Danaids was no longer understood.

PHOTOGRAPHIC SURVEYS

Mr EDGAR T. BOND writes :—

'In connection with some historical notes on Porthpean, in Cornwall, I am just completing a photographic survey of that village. This consists of about 70 photos, taken on the chain system—*i.e.* each exposure within the viewpoint of the previous one; it also includes several old ones taken about 20 years ago, and some from a photographic firm taken in the '80s. The latter show several cottages and paths now long disappeared.

'It soon became evident that to picture the village alone was not sufficient. The roads, paths and green lanes leading to it had to come into the plan wherever possible. Many may be altered or destroyed in the future and their present condition will then be of great interest.

'To make all this intelligible I am relating it to a large-scale map, about 12 inches = mile, showing the position and direction in which each picture was taken. Thus, no matter what the alterations, any particular place can be identified.

'All over the country there are members of archaeological societies who, with a little organization, could make similar records of these beauties of the countryside which are so rapidly disappearing. According to the Summer number of "Old Cornwall", the Falmouth branch of

the O.C. Socs. has already made a start. It is a chance to employ usefully a mass of dormant material that forms the "tail" of most such societies, and the more people you get to work the greater the interest.

'Any amount of such useful work could be done with an ordinary camera, but for a scheme such as I have detailed the cost would probably be prohibitive. With a miniature camera, however, it is quite moderate, and works out at about 50 exposures for £1—film, developing and " magnaprints " complete '.

THE CHAMBERED TOMB IN BEOWULF

In the great Old English epic of *Beowulf* the last exploit of the hero is, as is well known, a fight with a fire-dragon for the possession of a treasure which the latter guarded—a combat which resulted in the death of both. The poem gives a circumstantial account of the dragon's lair, which is described as beneath a great cairn (2213) *stanbeorh*, or a ' hoar stone ' (2744)—*under harne stan*. Within the cairn was a cave (2410) or treasure house (2279) the work of giants, and with stone arches or vaults, literally ' bows ', (2718) *stan bogan* resting on uprights. The dragon breathed out fire through an arch of stone (2545), and the outside of the lair is more than once referred to as a wall.

As Thurnam pointed out in 1869,[1] this is obviously the description of a megalithic chambered tomb. Stjerna, in his ' Archaeological Notes on Beowulf ',[2] written *c.* 1903–5, makes no mention of Thurnam and seeks to identify the description with a confused recollection of Roman buildings, inserted by the English redactor of the poem. ' The Beowulfine Lays, which related to the fight at the grave-mound, were thus expanded and blended in England with other lays, in which a certain awe at the Roman remains, with their wonderful vault-construction, is exhibited '.

The point we wish to make, which has not apparently been noticed hitherto, is that while Stjerna's Roman origin is obviously untenable, there is evidence that the description is an alien interpolation into the Scandinavian legend. This evidence is the mention of vaulting or corbelling in the megalithic tomb. Now such construction is an early feature in Northern chambered tombs, having its prototypes in the corbelled *tholoi* of Palmella and Algarve, but is quite unknown in the late megalithic architecture of Scandinavia. It occurs however not

[1] J. Thurnam, *Archaeologia*, XLII, 202–4; quoted with comments by O. G. S. Crawford, *Long Barrows of the Cotswolds*, 29–30.

[2] K. Stjerna, *Essays on Beowulf*, trans. Clark Hall (Viking Club 1912), 37 ff.

infrequently in the chambered tombs of Ireland and Scotland; New Grange and Maes Howe being outstanding examples. Both these tombs we know to have been broken into by treasure-seekers in early medieval times, for there is documentary evidence of the plundering of the Boyne tumuli in A.D. 861 or 862,[3] while the walls of Maes Howe are covered with runic inscriptions recording its violation by Norse raiders.

We suggest therefore that in the description of the dragon's lair in *Beowulf* we have an account of a megalithic tomb, not Scandinavian, but more probably Irish or Scottish. Such a description might have been compiled by the original bard from the accounts of raiders returning from these parts, or it may have been added in England by one personally acquainted with such monuments.

ALEXANDER KEILLER and STUART PIGGOTT.

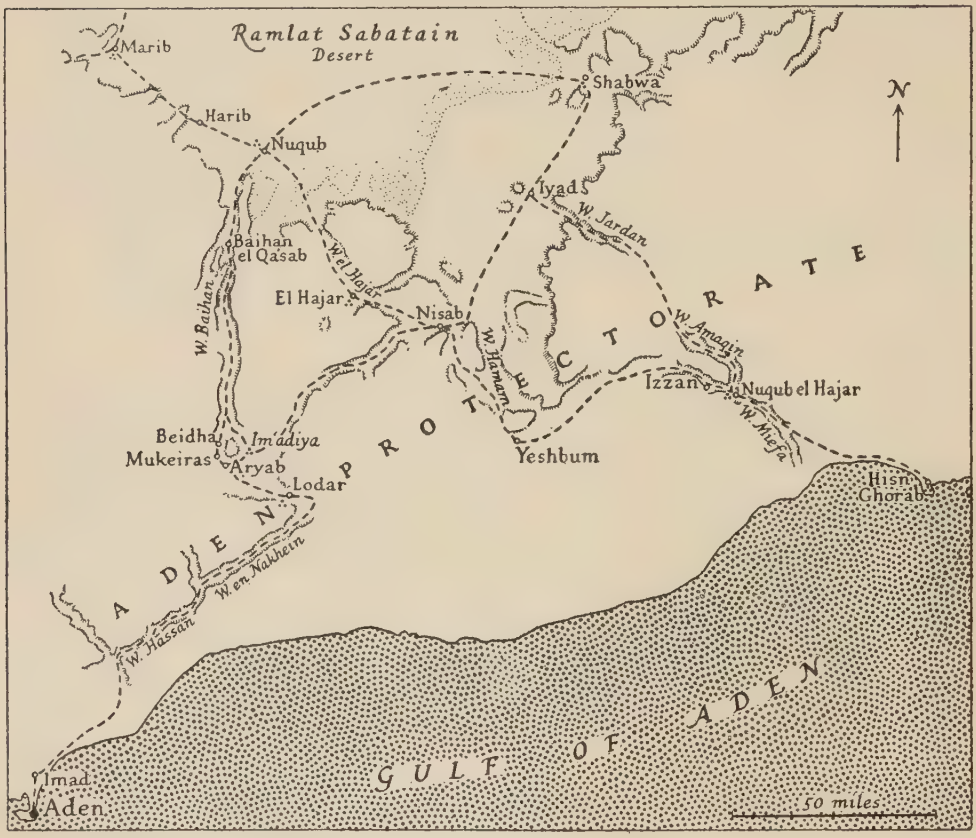
ROUTES IN SOUTHERN ARABIA

It is surprising that such a formidable corpus of inscriptions and proper names of the ancient civilization of South Arabia has been built up, while the practical basis is vague and often completely lacking. Semitic comparative philology is a study of great importance, but it must not be allowed to remain an end in itself. The most beautifully executed alabaster plaque is but a curio, and the names inscribed meaningless labels, if we cannot use them as historical documents, and get beyond the museum exhibit to the culture of which it is an expression.

An effort should be made to build up a working hypothesis, which would be of value even if in the end it proved wrong, provided it was based on the factor which has suffered least change—the country itself. If we take this view, it is more reasonable to make a start, not at the beginning with wild guesses and generalizations, but near the end of the long history where there is something concrete upon which we can get a grip. Once a small section of a road has been established, it is easy to travel either way. This figure of speech becomes almost literal if we agree that control of the fabulous Incense Route is the secret of the power and wealth of the early kingdoms. This indicates a study of communications, and if we can reconstruct a road-system from first-hand knowledge of the physical nature of the country, study of the inscriptions, and descriptions of the Classical geographers, we are in a position to take a first tentative step.

[3] G. Coffey, *New Grange*, 25.

The results of recent investigations in Wadi Baihan near the R.A.F. landing ground at Nuqub, and at 'Im'adiya indicate that the kingdom of Qâtabān flourished at periods in the early centuries of the Christian era. In his Geography,[1] Strabo writes that Kattabania lies



ROUTES IN SOUTH ARABIA

to the south of the Sabeaen territory, and from the capital at Timna' controls the country as far as the coast. The site in Baihan was known to Glaser and Rhodokanakis as Kohlân,[2] and they were satisfied from the evidence in the inscriptions which were found that it was Timna',

[1] Strabo, 16. 4. 2.

[2] N. Rhodokanakis, '*Die Inschriften au der Mauer von Kohlan Timna*'.

although Mr S. Perowne came upon an inscription which gave the name of the town as Baihan, when he visited the site in 1938.

This is not the time to enter into an academic argument on the two names ; what we can say is that there is very little doubt that it was the capital of Qâtabān, and it might be useful if we regard the area indicated by Strabo from a topographical angle. The great interest in the Nuqub site then lies in its strategic importance. If a military expert were handed a contour map with Aden, Marib, Shabwa, and Hisn Ghorab shown ; and asked where he would place a control point for communications, his choice would not differ much from that of the ancient builders. Situated at the bend where the Wadi Baihan begins to widen imperceptibly into the waste of the Ramlat Sabatain, it holds the fertile Wadi and the road south to Aden ; to the northwest Marib and the road to the north through Najran : to the east Shabwa and the road to the Hadhramūt ; to the southeast the road to Nisab and the coast. This is the basis of the accompanying map,[3] which despite its elementary simplicity incorporates the latest available knowledge.

The route from Baihan to Aden is of interest, as 'Im'adiya fits perfectly into the general scheme. Although the site has not been properly surveyed since Mr Perowne first made it known, his description and photographs give a definite impression of a strongly fortified hill-post. It commands the road where it leaves the relatively open Soma Plain for the narrow pass to Lodar, and is the key position on the Baihan-Aden road. It is also possible to go from 'Im'adiya to Nisab, but unfortunately little is known of this part. Between Lodar and Aden there is nothing of any importance known, except at Imad, about fifteen miles from Aden. On the ground it is a great sand-dune but from the air it assumes a definite shape, and the declination of the sand seems to indicate a straight road to Aden.

The names 'Im'adiya and Imad have a similar ring but it would be very unwise to deduce a connexion on that account. Although the traditional memory of the tribes is remarkable, there is very little to be learned from them concerning the ruins. Few of the sites have a significant local name ; hence the recurring ' El Hajar ', ' the Stones ' which is evidence of the complete lack of interest. On making enquiries about a stele in Baihan, the Sharif assured me that it was very old as it had been there in his great-grandfather's time !

[3] The map is on the International Scale, and wherever possible the readings of the standard maps have been checked by R.A.F. Survey readings, and in many cases corrected.

In Aden itself, there is no trace or justification of the flattering 'Arabia Eudaemon'[4] save in the famous 'Tanks'. Although the wonderful cemented rock-cisterns in Taweela Valley have been repaired and reconstructed beyond redemption by the British Authorities, a careful walk round Crater reveals traces of nearly fifty such interconnected cisterns—the remains of a gigantic water conservation system. The builders and the reason for their work, as well as the period of construction, are problems to which no one has attempted to give serious answers. Indeed it is questionable if the extent of the water-system has been fully appreciated. There are reports of cisterns and dams from all parts of the Aden Protectorate and the Yemen, and it is interesting to note how they conform to a set type, as though the principles of construction were the same for all. A comparative study would not only be interesting in itself, but might enable us to get an idea of the difficulties which the builders were facing. Then apart from the water question it is worth noting that even to an inexpert eye there is a striking resemblance in the finish of the cement work in the untouched cisterns in Aden, and the ruins in Baihan.

Although the Baihan-Aden road is an attractive prospect, it is nothing when compared to the great highway of the ancient world, from the south coast of Arabia to Petra in the North. This was the vein of gold which made Arabia Felix. Before we attempt to find the Incense Route, it is useful to lay down the major qualifications which we regard as essential. It should be the shortest practical distance; in a desert area, one with assured water and food supply; and from the all important aspect of security, one which can be protected.

If we attempt to follow the route in the Aden Protectorate, we must have two definite points to work from, which are Hisn Ghorab, the site of Cana, the sea-port where the road began, and Marib, beyond the Protectorate frontier. This is not assuming too much, as Hisn Ghorab has been recognized as the site of Cana by every reputable authority from Glaser onwards,[5] and the locality and importance of Marib is past doubt.

There is only one route which fulfils the necessary qualifications, and that is a road from Hisn Ghorab *via* Nuqub el Hajar, Nisab, El Hajar, Nuqub (Timna' or Baihan), to Marib. The first two depend on the natural features of the country and are still valid, but the security of the road is more difficult to establish. What we can say is that it was capable of defence and there are indications that it was actually defended. Hisn Ghorab is a fine natural harbour with extensive ruins

[4] Periplus, 26. [5] Glaser, 'Skizze der Geschichte und Geographie Arabiens', 174.

which are a rough indication of its ancient role as the frankincense port. The first ruins of any importance are at Nuqub el Hajar, and a glance at the map reveals their significance. This strongly fortified position, with its great water cisterns of the Aden type, is situated at the point where the road leaves the flat coastal plan for the narrow wadis. The next known fortified position is at El Hajar, between Nisab and Nuqub, where the wadi narrows and is easily defensible.

There is very little accurate information available on the road between Nuqub el Hajar and Baihan, but many conversations with Political Officers, R.A.F. Officers, and tribesmen from this area, confirm that the route is open, well watered, and still in general use. There are many stories among the Arabs of the Nisab section of ' written stones ', but as yet no investigations have been made.

In view of the fact that Shabwa has been identified with the Sabbatha of the ancients, the great centre of the incense trade, some explanation is due for its exclusion from the route suggested above. Shabwa was a magic name which could be transformed from Sabbatha to Sheba, and the legend was never seriously challenged until in recent years it has been visited and surveyed. The great city which Pliny pictures, with the sixty stately temples within the walls,[6] was not what Philby found, and there is no evidence of past greatness on this scale. Although it would be foolish to ignore the works of the Greek and Roman writers, from Eratosthenes to Pliny, it is equally so to regard their every statement as sacrosanct, and distort modern discoveries to fit their framework. There are no indications near Shabwa of any great road leading south to Hisn Ghorab. The shortest route would be through Iyad, and to Nuqub el Hajar *via* the Wadi Amaqin, but it is doubtful. Even if the road did go through Shabwa, there is no road to Marib except through the waterless Ramlat Sabatain, which is out of the question as a trade route, and in fact the only road from Shabwa is *via* the ruins in Baihan. Moreover Shabwa was a Hudramūt stronghold, and in the majority of the inscriptions in which this state is mentioned with Qâtabān, they are at war or on unfriendly terms.

Unless the route which has been outlined is completely wrong, a proper survey should reveal a great natural highway linked by traces of its ancient glory. In view of the great work which has been done in the Euphrates Valley and Palestine, it is to be regretted that the civilization of Arabia Felix should be left shrouded in sand, and we remain little better informed than the Greeks two thousand years ago. JAMES DUNCAN.

[6] Pliny, 6. 32.

Reviews

ASINE: RESULTS OF THE SWEDISH EXCAVATIONS 1922–1930. *By* OTTO FRÖDIN *and* AXEL W. PERSSON. *Stockholm* (*Generalstabens Litografiska Anstalt*), 1938. *pp.* 452. 52 KR.

English readers have every reason to be grateful to our Swedish colleagues for publishing this book in English. We trust that they will show their appreciation of this courtesy by purchasing the work. They will certainly get their money's worth. The five campaigns directed by Dr Frödin produced a wealth of material invested with the highest scientific value owing to the accuracy of observations on the conditions under which finds were made. As interpreted by Prof. Persson it throws a great deal of new light on the earliest civilizations of Greece and on their relations with Crete, with Anatolia and with the north.

In the Early Bronze Age Asine was already an important port. Bales of merchandize, sealed in Crete, were landed there in Early Helladic times. These sealings provide the best direct evidence for dating the E.H. period in terms of the Minoan system and so of our own reckoning. The stratification of the successive settlements provides additional evidence for Mainland culture-sequence. The latest E.H. floor deposits are sealed by a burnt layer, as at many other sites. Within the E.H. period itself the ceramic remains at least can confidently be distributed over three phases, the stratigraphical sequence of which is objectively illustrated by the sherd-count given on p. 201. The Minoan sealings and a stone seal, remarkably like a VIth Dynasty Egyptian specimen, are attributed to E.H.III (the possibility that the seal might belong to an earlier phase does not seem to be quite excluded by the observations actually recorded in the text).

The succeeding M.H. age is likewise subdivided into three phases whereas most American and English excavators have hitherto only been able to distinguish two. Persson recognizes an admittedly brief M.H.I. phase distinguished by the presence of hand-made Minyan ware, but a preference for shiny surfaces and the absence of matt-painted ware. The latter fabric first appears in his M.H.II which should therefore correspond to the M.H.I of Blegen and Wace. We are, however, like Persson himself, 'struck by the way in which the first eras of the Late Helladic period, L.H.I and L.H.II are presented by their characteristic pottery. Occupation layers proper, nowhere appear, and wherever they occur it is in connexion with sherds with simple, linear decoration of the kind looked upon as typical of L.H.III'. If there be in fact no L.H.I–II occupation-layers, does it not follow that M.H.III covers also L.H.I and L.H.II, *i.e.*, that

Minyan and matt-painted wares remained the normal fabrics current at Asine (as apparently at Eutresis too) down to L.H.III times while at other centres L.M.I–II vases were being imitated in the classical L.H.I–II pottery ? If so, the Asine succession alone could hardly be used in evidence for raising the beginning of the Middle Helladic period to 2000 B.C.

Within the chronological framework thus established, a number of new observations are recorded. As samples we can only mention the use of tiles for roofing in E.H.III, the presence of sherds of distinctly Macedonian pottery in the M.H.II–III horizon and of a new group of coarse wares which reappear in sub-Geometric graves. Persson's familiarity with Turkish museums has enabled him to recognize a close parallel to an E.H.I pot in the Copper Age of Alisar, a probable Early Hittite import in M.H.III and the (indirect) connexion between his M.H.II matt-painted ware and the Early Bronze Age pottery of Cappadocia.

In his very interesting conclusions Persson agrees with most other authorities on the Anatolian ancestry of Early Helladic culture. But the technique of glaze painting, attested first in E.H.II, would be derived from Crete. The intrusive culture of the Middle Helladic period would be due to the first Indo-European Greeks and derived from north of the Balkans. A parallel wave of invaders from the same quarter would have crossed the Dardanelles, whence some spread southwards coastwise while others advanced to Central Anatolia to form the Indo-European ruling class in the Hittite Empire. A reunion of the two streams would be reflected in the Anatolian traits recognizable in M.H.II–III (matt-painted ware, jar-burial for children). As northern traits in the M.H.I culture our author claims contracted burial, perforated axes, antler sleeves and picks, and clay seals. But to call contracted burial ' a Nordic feature ' (p. 349), is absurd. It suffices to mention Predynastic Egypt to invalidate the inference. Perforated axes seem older at Troy and Thermi than beyond the Balkans and at Malthi in Messenia are assigned by Valmin to E.H.III. The clay seal is not particularly like the Danubian and Thracian ' pintaderas ' which in any case are usually regarded as copies of Asiatic or Minoan stone seals.

Nevertheless Persson's thesis has much to recommend it. His observation that, in M.H.I and M.H.II, Minyan ware was made without the wheel does away with one of Forsdyke's arguments for bringing this pottery and its makers from Troy. Parallel inroads into Greece and the Troad would seem the proper solution. In any case the evidence from Asine is here set out objectively. The book contains many other facts and deductions which cannot even be mentioned here. The text is handsomely illustrated by three coloured plates, six folding plans and 275 text-figures. It thus forms a model publication worthy of a place beside the great American and English monographs that have been devoted to other Mainland sites.

V. GORDON CHILDE.

ZAKLÁDY HALLSTATTSKÉ PERIODY V ČECHÁCH (The Dawn of the Hallstatt Period in Bohemia). *By* JAROSLAV BÖHM. *Praha*, 1937. *pp.* 299.

The hill-girt basin of the Upper Elbe, interposed between the Danube valley and the great plain of northern Europe with its copper and tin and fertile lightly-wooded löss, was no less important in prehistoric than in recent times. It has always been a junction where natural routes converge, a coveted prize for barbaric neighbours. The relics that crowd the local museums faithfully reflect the contacts with other regions established by the trade and industry of its population or by brutal aggression from outside. Monographic studies of this material by Czech prehistorians, unbiased by any 'Nordic mythology', have therefore always been at the same time authoritative contributions to general European prehistory. While the political boundaries of Czechoslovakia still corresponded to those fixed by nature, Dr Böhm was able to compile such a monograph on the dawn of the Hallstatt period that is a worthy successor to those of Stocký, Schránil and Eisner on the neolithic, and Early and Middle Bronze Ages. His conclusions, based on a thorough analysis of all the collections (largely unpublished) are of first-class importance for such issues as the absolute chronology of the Bronze Age and the origins of the Kelts. Not only the Knoviz pottery here described, but also the shepherd's crook pins associated with it, have a direct bearing on British archaeology.

Bohemian prehistory is here depicted in a complexity which comes as near historical reality as archaeology is likely to get. The country is subdivided into four regions, the parallel developments in, and interactions between, which have all to be considered. When the study opens about 1400 B.C. (Reinecke A2) the Aunjetitz culture was firmly established in central as in northern and eastern Bohemia, but the tumulus-culture was already emerging along the Danube-Elbe watershed to the southwest. The connexions of the latter lie south of the Danube in the Sudeten-Danubian province (even the flutings on advanced tumulus pottery can be derived from the southeast whence it would independently have come to Lausitz and southwest German ceramics). Five stages of development can be distinguished. By the third stage ('Kbely-Řepeč') the tumulus-culture was spreading into central Bohemia to be superimposed upon the Aunjetitz culture. Remnants of the Aunjetitz population doubtless survived, but they made no recognizable contribution to the local culture of the period (Reinecke C 1225–1100 B.C.) which is completely assimilated to that of the tumuli.

After 1100 (Reinecke D) the development of the mixed culture in central Bohemia is disturbed by the intrusion of the Lausitz culture. The latter originated, according to Böhm, north of the Riesengebirge between the Elbe and the

Warthe with an actual infiltration from the Sudeten-Danubian province through the Moravian Gates to impinge upon the Silesian Aunjetitz culture. The Lausitz culture emerges in northern Bohemia even before 1100 B.C. (in Reinecke C). Then, superimposed on the central Bohemian tumulus-culture with its suppressed Aunjetitz survivals, it produces a new hybrid, the Knoviz culture.

In central Bohemia the latter develops through four phases to culminate about 700 B.C. in the explicitly Iron Age Bylany culture. But the Knoviz, like the Lausitz, culture exhibits a strong tendency to expansion. Spreading southwest, in the wake of an earlier and rather vague Lausitz stream, it overlaid the local tumulus-culture, transforming it into the Milaveč culture. The Milaveč culture in southwestern Bohemia reaches the zenith of its development in Hallstatt A between 1000 and 850 B.C. and thereafter seems to vanish. But beyond the divide, the expanding Knoviz culture, superimposed upon local branches of the tumulus-culture, can be traced as a principal component in the southwest German and Tyrolese urnfields. An independent expansion of almost pure Lausitz culture is, however, admitted as responsible for the urnfields in the Main valley and the Middle and Lower Rhine.

By a revision of connexions with Italy and the Aegean Böhm justifies a short chronology. The later phase of the Aunjetitz culture (Reinecke's A2) begins about 1400 B.C., a date which could now be justified by Beck's and Stone's dates for segmented faience beads, but must last till nearly 1200. Sub-Mycenaean parallels to the violin-bow safety-pins from Lower Austrian and Tyrolese urnfields put the last phase of the pure Bronze Age (earlier Knoviz) between 1100 and 1000 B.C. Spectacle-brooches such as first appear in Hallstatt A (later Knoviz) are dated in Thessaly by Heurtley between 1000 and 800 B.C., while the antennae swords and crescentic razors of the succeeding period are assigned in Italy to 850–700 on the chronologies of Åberg and Ducati. The beginning of the succeeding Hallstatt C–D Bylany culture about 700 B.C. is deduced from Arnoaldi imports, while its end by 400 B.C. follows from Jacobsthal's dating of the succeeding La Tène phase. In an interpretative section at the end the author asserts that the battle-axe folk, in a wide sense, are to be identified with Indo-Europeans and argues that central, southern and western Bohemia should be regarded as forming parts, albeit peripheral parts, of the Keltic cradle.

The Czech text is accompanied by a German resumé of 33 pages but a fuller German work treating of the period throughout the Republic is promised. Now it would seem eminently desirable that the present book, perhaps the last account of a controversial period that can appear in Central Europe unaffected by politically inspired dogmas, should be translated *in extenso* into English. In that case some modification of the terminology for the benefit of non-Slavonic readers

might be advisable. Here each phase in the evolution of four cultural series is normally denoted by a pair of Czech place-names, like Drhovice-Vrhaveč. A foreigner may be expected to learn the names Knoviz, Milaveč, Bylany, as he has Aunjetitz and Lausitz. The phases of the cultures thus designated could surely be indicated by the addition of letters or numbers. The maps might also be printed on a rather larger scale. V. GORDON CHILDE.

THE POTTERY OF SANTO DOMINGO PUEBLO : a detailed study of its decoration. *By* KENNETH M. CHAPMAN. Memoirs of the Laboratory of Anthropology, vol. I. *Santa Fé, New Mexico*, 1936 [received Dec. 1938]. *pp.* XIV, 192, 80 *plates*, 34 *text-figures*. *Price* $4.00.

A detailed study of this interesting pottery, which the author has been collecting for nearly twenty years. Mr Chapman lays particular emphasis upon the fact that Santo Domingo decorative art has not been influenced by contact with Europeans. ' Pueblo pottery of the past three centuries ', he says, ' proves to be a normal development, in which scarcely a trace of European contact can be detected '.

The author's description of the decoration he has so carefully studied perhaps suffers from too much analysis. I mean that it considers perhaps in too abstract a way the decorative elements as single units, first isolated, then arranged and combined by the potter's skill. Nevertheless, it seems quite conclusive as to the stability of the traditional Pueblo art ; it shows, too, that a certain margin of adaptability to new types of life and new techniques may give promise of a survival of indigenous pottery-making. In spite of the well-known Pueblo conservatism, some innovations have appeared. Mr Chapman points to the real problem which is now in view, that is, the necessity of giving this craft a sound economic basis, the lack of which accounts for the decline of Zuñi pottery. Interesting efforts are now made in the southwest United States to resolve this problem.

Mr Chapman also gives much useful information on the methods used for pottery-making and painting, and upon the symbolic meaning of some decorative elements. JACQUES SOUSTELLE.

THE ' RAIN BIRD ' : a study in Pueblo design. *By* H. P. MERA. Memoirs of the Laboratory of Anthropology, vol. II. *Santa Fé, New Mexico*, 1937 [received Dec. 1938]. *pp.* 113, 48 *plates*. *Price not stated.*

This is a very careful study not only of the so-called ' Rain-Bird ' motive, but of the evolution of Pueblo pottery from the standpoint of decoration. The writer shows how the elaborate decoration of the historic forms gradually evolves from the rudimentary stages of the prehistoric times. Generally speaking, it

seems that pottery appeared in the southwest of what is now the United States during the early centuries of our era (' Basketmaker III period ' as it is technically known). At this stage, decoration consisted only of lines and dots. Mr Mera traces the evolution from those crude motives to the refined decorative system used by the Zuñi, Hopi and other Pueblo potters.

The ' Rain-Bird ' motive is one of the most generally used by the Pueblo, as it is found practically in all their towns. The author is quite right, I think, in emphasizing the effective and traditional value of the decorative art in a tribe where the greatest conservatism still prevails. The plates are quite good.

JACQUES SOUSTELLE.

DER WEG AUS DEM CHAOS. *By* PAUL LIGATI. *Callwey, Munich*, 1931 [received 1938]. *pp.* 296, 317 *figs.*

The idea of a cycle of repetition in civilizations was known 2000 years ago to the Etruscans (Plutarch, Sulla). The study of early civilizations in Egypt had, about 30 years ago, led me to a similar result, explained in *Revolutions of Civilisation*. At that time the glamour of universal progress, started in the Exhibition of 1851, made any idea of a reversal heretical. We know better now in Europe. *The Downfall of the West* by Spengler was startling ; it is now a commonplace.

The most detailed exposition is by Paul Ligati of Buda-Pesth. In 296 pages of close text there are, however, no references given to the 317 illustrations which follow. The figures are so much diversified, from Modern to Ancient Egyptian, that it seems impossible to recognize a clue to their purpose, in the absence of any references to the text.

The many diagrams provide a wavy outline of civilization, with names in every part. In music, Handel and Wagner are on a rise, and Mozart and Beethoven in a fall, the application of which is obscure. The Venus of Milo, by the way, is entered a couple of centuries too late. As in different diagrams the length of periods varies from 70 to 150 years, no general system in civilization can be demonstrated.

The conclusion is that it is the mission of Genius to receive from the heights and bring down to mankind—a task, a moral duty, towards the remainder of mankind.

FLINDERS PETRIE.

THE CAMBRIDGE REGION. *Edited by* H. C. DARBY. *Cambridge University Press*, 1938. *pp.* XIV, 234 ; 58 *text-figs.* 6*s.*

The meetings of the British Association at Cambridge in 1904 and 1938 were both marked by the issue of a scientific survey of the Cambridge district. The earlier volume, the well known ' Handbook to the natural history of

Cambridgeshire ', edited by Marr and Shipley, described the geology, zoology, botany and prehistoric archaeology of the county, while in the later volume here under review thirty-four authors have covered the same field and included much else besides. A comparison of these two books affords an interesting commentary on the progress of research in the last generation.

The chapter on geology and physiography is based, like other sections, on material prepared for the recently published first volume of the *Victoria County History of Cambridgeshire*. Here the most notable additions to knowledge concern the underground structure of the district, especially the nature and depth of the Palaeozoic floor now being studied by seismic methods, a line of research unknown in 1904, and the post-glacial deposits of the Fenland where detailed exploration has begun with the combined efforts of archaeologists, botanists and geologists. Accounts of the soils and climate, not included in the 1904 handbook, are valuable contributions in a district so largely agricultural. The main types of top soils are clays from boulder clay or Jurassic and Cretaceous rocks, chalk on the northeast-southwest extent of downland, and fen silts and peats in the north half of the county. This distribution controls not only the types of farming, noted in the chapter on agriculture, but also village-sites (compare text-figs. 24–25 with text-fig. 4). The heavy clay soils are the least productive; on the chalk belt farms are large and sheep the livestock traditionally associated with this district, while in the Fenland production is much more intensive, especially on the extremely fertile alluvial silts. Half of the botanical chapter is devoted to the Fenland, in many ways the most remarkable area in the Cambridge region. Here there is a great advance on the 1904 account, which consisted mainly of lists of plant-associations. The increase in knowledge in the last 30 years now permits an ecological approach. The natural vegetation successions can best be studied in those communities not seriously altered by agriculture, and Cambridgeshire is fortunate in including more of the peat fens than any other county and in particular Wicken fen, perhaps the largest area of uncultivated fenland. The zoological section omits the long lists of species in the corresponding part of the *Victoria County History*, and in so doing gains for the general reader who seeks an up to date summary of the fauna of the county.

It is in the archaeological chapter that the contrast with the 1904 handbook is most marked. A generation ago regional distribution studies had not begun, and the account of the prehistoric archaeology of Cambridgeshire described the trackways, the defensive dykes and finds of small objects. The work of the subsequent thirty years, which includes the distribution studies of Sir Cyril Fox, the investigations of the Fenland Research Committee on the post-glacial sequence in the fens, and of the Cambridge Antiquarian Society on the great

dykes and Anglo-Saxon cemeteries, has far more than local importance. The relation between the areas of human habitation and land movement is well illustrated by the distribution diagrams, which show a retreat from the Fenland between the Bronze Age and Early Iron Age. In the Roman period there was a re-advance into this district, where much of the silt area in the north of the county was intensively cultivated. This is almost the first account to be published of the great Roman agricultural area and its recognition completely alters the picture of Roman economy in this part of the country. The extent of the visible earthworks of these Roman villages and the ease with which they can be traced by air-photography is not generally realized.

The concluding chapters are devoted to Cambridgeshire in the nineteenth century, the growth of Cambridge and a description of the Breckland. The account of the draining of the fens contains much information about a subject prominent in recent years. Extension of the drainage with consequent shrinkage of the peat and progressive lowering of the surface seems likely to cause the Ouse Catchment Board more trouble in the future, unless the outfalls can be improved by promoting natural scouring of the river mouths.

This book is a fine example of a regional study produced by a team of writers. It provides a most useful summary of the Cambridge district, and the University Press is to be congratulated on its re-issue, which makes it available to a wider circle than members of the British Association. It is to be regretted that the geological map, also specially prepared for the Association's meeting, is not republished with the volume, which includes no large scale map of the district that it describes. K. St. Joseph.

LA PRÉHISTOIRE. *By* A. Vayson de Pradenne. *Paris : Librairie Armand Colin*, 1938. *pp.* 224, 47 *figures. Price ascertained to be* 15 *francs.*

This book is volume 210 of the ' Collection Armand Colin ', a series of popular manuals which aims at giving clear and precise expositions of the present state of knowledge in a very wide range of subjects. In the present volume world prehistory down to the close of the Neolithic is dealt with in 220 pages, of which 72 are devoted to method and technology, so the treatment is necessarily very summary indeed. The book is divided into three sections. The first, which is introductory, deals with definitions and programme, method, the search for prehistoric remains, and technology, and is probably the most valuable part of the work. Considering the limitations of space imposed on the author it contains a remarkable amount of information, presented with great clarity and economy of words. The chapter on Technology obviously owes much to M. Vayson's own technical training ; the descriptions of the process of working flint and other stones, and of the different types of tools, are much

more intelligible than is usually the case in text-books of this kind. The second section, Classification and Chronology, which deals with Western Europe, is also very clear, but suffers more from unavoidable compression. The chronology of the Lower Palaeolithic, for example, is a highly complicated problem, and although M. Vayson states very lucidly his personal views, it is hardly possible for him to do justice to those of other schools, and in particular of the Abbé Breuil and the group which works in collaboration with him, and he does not seriously attempt to do so. The last section, which deals with discoveries outside Europe, though again very much compressed, is admirably up to date ; this is probably the only small manual which attempts to include the latest discoveries all over the world.

Within the limits imposed by its size, and by certain predilections of its author, this book should provide a useful introduction to prehistory for the public to whom it is addressed. D.A.E.G.

LE PALMIER-DATTIER ET LES ARBRES SACRÉS DANS L'ICONOGRAPHIE DE L'ASIE OCCIDENTALE ANCIENNE, *par* HÉLÈNE DANTHINE. *Text, pp.* 279, *album of plates with* 1217 *figures. Paris : Paul Geuthner*, 1937 (received November, 1938). 300 *Francs.*

This admirable work is produced in a manner worthy of the importance of the subject treated. The excellent types selected for the text and the well-spaced pages make reading a pleasure, and the 1217 illustrations, mostly in line-drawing, are clear and adequate for the purpose. They are, indeed, so numerous and cover such a wide range of ancient art that it would be difficult to find a representation of a sacred tree which is not included. Scholars will feel an immense debt of gratitude to Dr Hélène Danthine who, by her thorough and penetrating analysis of the available material, has spared them the trouble of further search for examples.

Very few ancient texts treat of the sacred tree apart from a text descriptive of a New Year ceremony, the legend of the dark *kiškanu*-tree which grew in Eridu, and a few scattered allusions. But in this book the brilliant analysis of all the evidence shows how much can be learnt even from such meagre sources.

The title of the book gives an accurate indication of its contents. The tree considered in its religious significance was ' sacred ', but it was not a ' tree of life ' in the sense that to eat of its fruit could confer immortality. The palm-tree is rightly named first in the title, for it was the sacred tree *par excellence*. For this reason the authoress begins with a valuable botanical study of the palm-tree, its area of diffusion, its characteristics, and the methods employed in its cultivation. This careful investigation compels her to reject both the widely-held theory that scenes in which genii hold a cone and a pail represent an actual

fertilization of the sacred tree near which they stand, and the alternative theory which would explain the scenes as a ritual aspersion. She concludes that any idea of realism is excluded by the artificial character of the tree and the aspect of the genii, and believes that the scenes can only be symbolic representations of artificial fecundation.

A detailed examination of the forms, sometimes exceedingly abstract, which the sacred tree assumed in the course of centuries is followed by a discussion of the scenes in which the tree figured. One of the problems of the subject is to decide whether any given delineation of vegetation depicts a sacred tree, or is merely a tree as an indication of landscape. It is true that some scenes, which seem to give no indication as to whether they have or have not a religious meaning, are proved by comparison with more complete or precise specimens to fall into the category of sacred trees ; in this book every tree has been given the benefit of the doubt to avoid the omission of an example which might by a remote possibility be classed as sacred. Professor Walter Andrae has pointed out (OLZ 1938, s.508f.) that three phases of the sacred tree are represented, namely, with bare twigs, with leaves and flowers, and, lastly, laden with fruit, thus corresponding to the three functions of life.

The authoress describes plants of diverse species which were sometimes pictured apparently in the place of the date-palm. But she offers no explanation as to why the conifer was so largely represented during the Akkadian period, and to a lesser extent in the Larsa period, and whether it had exactly the same significance as the date-palm. It seems probable that the date-palm and the conifer both implied ' fertility ', but that, as the fundamental idea which they symbolized was not identical, both of them signified in addition particular attributes derived from the sources from which they originated.

A description of the compositions in which the tree figured leads inevitably to an attempt to interpret their meaning. Mlle Danthine ably summarizes the views held by various authorities on the subject, and this gives her an opportunity to consider the connexion between the sacred tree and the two trees of Paradise, the tree of life and the tree of truth (E. Dhorme, *L'arbre de vérité et l'arbre de vie : Revue Biblique* (1907) IV, 271–4 ; A. Ungnad, *Die Paradiesbäume : Zeitschr. der Deutschen Morgenländischen Gesellschaft* (1925) LXXIX, ss. 111–118). In this respect it might have been explained that the reason why a single tree or, still better, two trees are frequently introduced into scenes where two divine attendants open the wings of a door or hold a gate-post is because they typify the two trees which stood at the door of Paradise, the *giš-ti* and the *giš-zi-da*, guarded by the two guardians of the gate of Anu's palace, Dumuzi and Ningizzida.

Upon a subject so vast and complicated, and bristling with such thorny

problems, no one writer can be said to have pronounced the final word. But in this comprehensive and admirably arranged study Mlle Danthine has offered to all who are interested in the subject an inexhaustible fund of material and clearly presented information which scholars will find indispensable, for all will gladly acknowledge the great merits of this remarkable work.

E. Douglas Van Buren.

SARDINIAN STUDIES. *By* Members of the Le Play Society. *Published by the Society* 1938. *pp.* 60 *and* 12 *plates.* 2*s.*

Sardinia is rich in exceptionally well preserved and imposing prehistoric monuments, and has moreover yielded a vast number of curious bronze figurines of the warriors who built some of the defensive constructions. To British archaeologists the monuments are of especial interest owing to the remarkable similarities some exhibit to certain groups of Scottish and North Irish remains—the Horned Cairns and the Brochs. A useful English introduction to their closer study, together with notes on the geology, fauna and flora of the island and its later ecclesiastical architecture, is provided by the handy little volume of essays written by members of the Le Play Society who visited Sardinia in 1934.

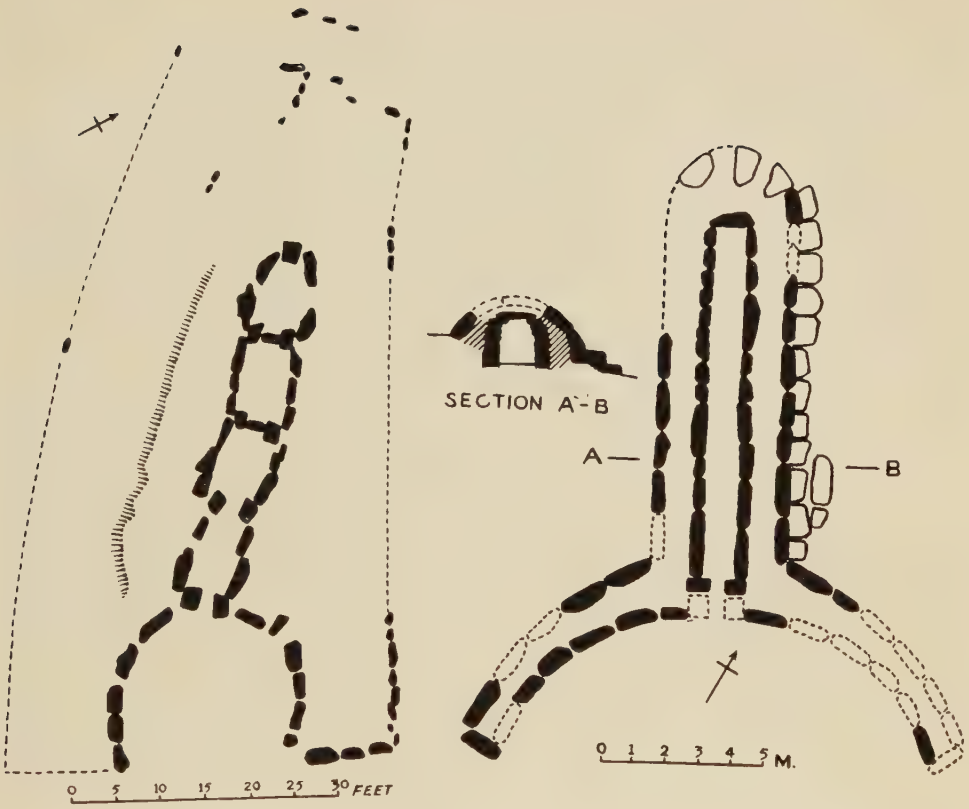
The megalithic gallery-graves, termed locally *tombe di giganti*, or Giants' Tombs are extraordinarily like the horned cairns of North Ireland and Scotland as Bryce noted in 1902. But plans are needed to make the comparison intelligible (PLATE I). And then it must be remembered that, while the British tombs are built of rude blocks, the façades and even the chambers of *tombe di giganti* are very often formed of dressed slabs. In classical specimens the ' portal ' takes the form of a dressed and carved stele at the base of which a diminutive port-hole opens into the chamber (see PLATE II). This stele is disproportionately high in comparison with the chamber and the rest of the façade. But high portals, composed of undressed monoliths set very close together, are also a feature of the Scottish horned cairns as defined by Bryce. The latter, of course, are standard monuments of the ' Stone Age ', but the stones of the Sardinian tombs seem to have been dressed with metal tools.

But in Sardinia, we are told, the *tomba di giganti* developed out of the simple dolmen[1] . . . that is, a large flat stone resting on uprights to cover the place of burial '. I saw a classical ' dolmen ' last January beside the railway at Birori[2] near Macomer. Viewed close up the single table supported by seven uprights conforms perfectly to the usual definition. Examination of the vicinity,

[1] This thesis was worked out in detail by Mackenzie (*Papers of the British School at Rome*, V and VI).

[2] A plan of the ' dolmen ', as in PLATE III, is published in *Bulletino de Paletnologia Italiana*, 1906, XXXII, 268.

PLATE I



PLANS OF MEGALITHIC GALLERY-GRAVES, IRELAND (LEFT) AND SARDINIA (RIGHT)

(see p. 376)

facing p. 376

ROCK-CUT TOMB ('WITCHES' HOUSE') NEAR SASSARI, SARDINIA (*see* p. 377)

PLATE IV

however, discloses other stones and in particular three set on a semicircle just like the foundations of the façade of a normal *tomba di giganti* (PLATE III). Close by the gallery of such a tomb is preserved complete with five uprights along each side and one capstone still in position. There are vague traces of a third tomb too. It really looks as if this ' dolmen ' in any case is just the best preserved section of a similar gallery-grave—in fact of a typical *tomba di giganti* ! To assume a priority of the ' dolmen ' over the Giants' Tomb on such evidence would seem unjustifiable.

On the other hand some *tombe di giganti* are clearly contemporary with some of the rock-cut tombs, termed locally *domus di gianas*, Witch's Houses. Over the doorway of two such rock-cut tombs near Sassari a faithful reproduction of the stele, that forms the entrance to the built tombs, has been carved. The rock-face on either side of the door has been quarried away and smoothed till it is concave like the built façade on either side of the portal of a Giants' Tomb (PLATE IV). This provides a convincing example of the interchangeability of rock-cut and megalithic chamber-tombs. It would be interesting to plot the distribution of the two types on a geological map—since the method of construction is presumably determined by the nature of the local rock. But the geological map in *Sardinian Studies* is too small for this purpose, and no adequate distribution map of the monuments has even been published—that given by the Le Play Society does not even distinguish between built and rock-cut chambers !

Be that as it may, while some Witches' Houses are Iron Age, others, like those at Anghelu Ruju, go back to Beaker times or earlier. And even these may have the doorways carved, though not with ' stelae '. So some Giants' Tombs too may be Copper Age. Their distribution does indeed approximate to that of the (late) ' Bronze Age ' *nuraghe*, but *nuraghe* are too widespread for that to mean much. There is no *nuraghi* at all near the Birori group of tombs. Though the two tombs at Geronna stand on the same plateau as a ruined *nuraghi*, the latter is built over the still more ruined wall of an earlier enclosure.

The *nuraghe* themselves—those towers of dry masonry that from without look so like the Scottish brochs—are adequately described in the book. An illustrated appendix is devoted to the solid-wheeled carts[3] still in use in the mountainous district round Abbasanta and Macomer. Like the bullock-carts

[3] Illustrations of solid-wheeled carts still in use have been published in ANTIQUITY, 1929, III, 340–1 (Sardinia and Spain), and 1931, V, 197–9 (India). Carts with solid wheels of precisely the same type as the Spanish ones were still used in England in the 18th century. Pococke saw them at Penrith in Cumberland in 1760 and thus described them : ' The wheels and Axel trees of their carts turn together, and the wheel consists of three pieces of wood ; a small segment of a circle being cut out of the two side pieces and a little from the middle piece ' (*Tours in Scotland*, by Richard Pococke, Scottish History Society, 1887, vol. I, 36). A solid wheel of this type found in Tindbaek Mose, Denmark, is illustrated in Brønsted's *Danmarks Oldtid*, 1938, fig. 93.—EDITOR.

of Sindh, they illustrate much the same type of wheel as is represented on the ' Standard ' from the ' Royal Tombs ' at Ur and contemporary early Sumerian documents. This Oriental type, introduced into the island in prehistoric times, has been preserved, because, we were told, the solid wheels stand the wear on roads of naked jagged rock better than spoked ones. V. GORDON CHILDE.

BRITISH CORACLES AND IRISH CURRAGHS. *By* JAMES HORNELL. *Bernard Quaritch*, 1938. 46 *plates and text-figures.* 7*s* 6*d.*

Mr Hornell's work on the typology and evolution of boats is well known to readers of ANTIQUITY. With this book he places the archaeologist, and all others interested in the survival of the past into the present, deeply in his debt. As happens not infrequently in enquiries of this kind, it is left to one whom the extreme nationalist would label a ' foreigner ' to record and interpret cultural features which are unique in Europe. Here, for the first time, is a comprehensive survey of the wicker-work craft of West Britain and Ireland ; their distribution, varieties, modes and materials of construction, uses, methods of carrying and propelling, types of paddle and oar, terminology and customs associated with the craft, their history and probable lines of evolution and devolution. An admirable series of photographs and scale-drawings rounds off the study.

The author believes that the British coracle, usually round or oval in shape, is a far older form than the boat-shaped curragh, which was ' evolved in Celtic Britain from a mixed ancestry, its form modelled upon that of ancient plank-built boats '. The former, he holds, is derived from the same common source as the *quffah* of Iraq, a chapter on which is appended. All Irish vessels of this class, including the oval Boyne type, are built bottom upwards, and it is partly for this reason that Mr Hornell would attribute the Boyne curragh to a process of degradation from a sea-going form. One would like to know what kind of craft was used in pre-Celtic Ireland. Irish archaeology may help to solve this important problem if it can discover curragh-remains in the peats, and extensive excavation of one of the prolific Bann sites is an urgent requirement. Paddles are already known from the Bann valley.

The book has two minor defects consequent upon its origin as a reprint of a series of articles which appeared in *The Mariner's Mirror*, the Quarterly Journal of the Society for Nautical Research, although it must be said that its attractive format conceals its origins. The first defect is that the pagination and figure-numbers are not consecutive, and the second is that the general reader is often held up by technicalities which would no doubt be familiar to members of the Society. A short glossary would have helped the not inconsiderable number of readers who have no first-hand knowledge of boat-building. We

welcome the news that the surviving types of Irish fishing craft, apart from the curraghs, are being studied, and we strongly recommend this book not only for its own sake but also because, as Sir Geoffrey Callender tells us in his introduction, any profit on its sale goes towards the cost of the Irish survey.

ESTYN EVANS.

THE DISCOVERY OF MAN ; the story of the inquiry into human origins. *By* STANLEY CASSON. *Hamish Hamilton*, 1939. *pp.* 339. 12*s* 6*d*.

Histories of archaeology and anthropology may easily become unreadable catalogues, where great men and small are each allotted the same amount of space. This is not that sort of book (none of Mr Casson's are). It is at its best when the author is writing the history of his own special branch of archaeology ; but it is readable even when he is describing the rather dreary history of discovery in South America. Certain new high lights emerge of which we had never even heard. The fascinating story of Cyriac of Ancona (one of them) is admirably told. We are not quite sure that the story of Boucher de Perthes is quite fully or accurately given, though Mr Casson's account is clearly correct in most parts. Sir John Evans and his colleagues *were*, surely, satisfied that, in spite of occasional salting, genuine implements *had* been found in position in the Somme gravels. Some years ago Sir Arthur Evans mentioned to me in conversation that he had himself, as a boy, accompanied his father on one of his expeditions to France. Impressed by the interest of the living link thus provided with the Heroic Age of modern archaeology, I asked Sir Arthur to set down the chief facts in writing, and this he kindly did, allowing me at the same time to publish them. In a letter dated 18 May 1936 he describes the extent of his ' personal knowledge of the chapter of palaeolithic discovery that began in 1858. In that year ', he wrote, ' encouraged by Dr Falconer's good opinion of Boucher de Perthes' implements . . . my father (Sir John Evans) joined Professor Prestwich in actual researches in the S. Acheul and Amiens gravels ; on which occasion, as you may remember, they actually found a flint implement *in situ* 17 feet down. I was then only a child of 7, but I recall the domestic reactions and both Falconer and Prestwich very well. It was only in January 1865 that I was permitted to accompany my father in a journey to France. Our first quarters, as I remember well, was Béthune, and we made a round of the principal Somme valley sites, and saw the French people concerned there and in Paris ; thence going down south to see Marquis de Vibraye's wonderful collection of cave specimens. I also remember the Abbé Bourgeois' collections at Pontlevay. I was then 13½, but I quite entered into the atmosphere of those days '.

I have kept this letter till now, waiting for a suitable occasion to print it, and I do so now on account of its great interest, and not so much in criticism of

Mr Casson's account as to supplement it. It is pleasant to record such a link with those far-off days, and to know that the great archaeologist who remembers them is still amongst us. Mr Casson describes in chapter 5 (Great Discoveries), Sir Arthur Evans' own role in the making of archaeological history, of which it may truthfully be said that in archaeology it is the finest single-handed achievement of all time.

The author plainly sees—what so many overlook—that modern archaeology dates from the moment when *things* were made to tell their own story, to reconstruct the past without regard to documents. I think he might have enlarged a little more upon this interesting theme ; the change-over in attitude marks the beginning of scientific archaeology as opposed to antiquarianism. I am still a little puzzled about it myself—how and when did it occur ? Generally speaking, about the beginning of the 19th century, of course, but it seems to have crept in silently. Perhaps here again it was Boucher de Perthes' discoveries that were the first outstanding instance of the new method of approach. In England Sir Richard Colt Hoare (who should be mentioned—he was a far greater man than the collector William Greenwell) very nearly attained the modern outlook ; he was, I think, the first to publish a stratified section of an earthwork (Wansdyke) and to draw conclusions from it ; and he certainly drew conclusions from his barrow-finds and British villages.

Aubrey was, however, a good and indefatigable field archaeologist in the century before Stukeley ; his observations of earthworks, recorded in the still unpublished Monumenta Britannica, prove this. He was also the discoverer of Avebury ! The plan of it which he has left is by no means useless, though neither so full or on so large a scale as Stukeley's.

Schliemann is rightly given the place of honour which is his due. Speaking once with the veteran Professor Sayce I asked him—who had watched the whole story of modern archaeology develop, ' et cujus pars magna fuit '—with whom, in his opinion, it had begun ; he replied, without a moment's hestitation, ' with Schliemann '.

After Schliemann, and in another field, General Pitt Rivers was the outstanding figure. He surely deserves more than the single and not very adequate paragraph on p. 278 ; for it was he who introduced scientific method into the excavation of prehistoric sites in Europe. Pitt Rivers' influence has affected every living excavator in Britain and he is still (as in my day) respected by members of the ' younger school ', who are not in any generation prone to be tolerant of their elders (and often with justification). So excellent were his methods of record that fresh information can still be extracted from his excavations as new problems arise, and new demands are made.

I conclude with a hearty commendation of an interesting and valuable book, and the usual irritating list of quite minor points.

Page 182, Ightham is situated not ' in ' but *near* ' the rolling chalk hills of Kent '. It stands on the Lower Greensand.

Pages 287, 288. The evidential value of the Kentish eoliths is NIL. Well attested *palaeolithic* implements have been found in the highest plateau gravels of Kent, and prove the existence of man at the time when those, the oldest, gravels were deposited. The game being thus won by playing the higher card, the lower ceases to be of any value as evidence.

Page 300. The palaeolithic is the period of hunting rather than of food-collecting, which is characteristic rather of the marginal Europeans of mesolithic times.

Page 317. One certified discovery *appears* to have been made, but it is still not published, so far as I am aware, and until then judgment must be suspended (see ANTIQUITY 1938, XII, 232).

Page 320. The best modern opinion inclines to the view that America was first reached from Asia *via* the Aleutian Islands rather than across the Behring Strait.

Page 326. Omit France. O.G.S.C.

HISTORY AND ROMANCE IN GRAECO-ORIENTAL LITERATURE. *By* MARTIN BRAUN. *Basil Blackwell*, 1938. *pp.* 106. *7s 6d.*

This is an interesting treatise on that curious type of literature, half folk-literature and half legend and history, which grew out of the main body of Greek and Oriental writing. As literature it has almost no value ; as a special and inner view of the Oriental mind it is of importance. The stories of Semiramis and Ninus are analyzed by the author with care and scholarship. The interaction in Greek and oriental story-telling of the original myth is explained. Sesostris, the Egyptian national hero, is shown developing as a nationalist saga when Egypt was under Persian rule. Fragments of Greek history creep into the general story. Nectanebus, the last Egyptian native Pharaoh, was another national hero of story and legend.

Moses also figures as an Egyptian hero, originally a priest of Heliopolis. Finally Alexander is discussed—Alexander about whom more legends accumulated than of any other person in history. The author also deals with the Potiphar story and its ramifications in Jewish and Hellenistic legend.

The work is an interesting one, for it shows literature in decay and legend in the making. S. CASSON.

AIGINA. *By* GABRIEL WELTER. *Berlin : Archaeologisches Institut des deutschen Reiches*, 1938. *pp.* 134, *and* 87 *figures.* RM. 5.10 in England.

This is one of an excellent if small series sponsored by the German Institute. ' Priene ' is a similar handbook. Gabriel Welter has long established himself

as the leading authority on the island of Aegina, and his patient excavations have contributed largely to its prehistory. This book, excellently produced and illustrated, gives the salient points in Aeginetan political history and art, from the Neolithic Age to the present day. Aeginetan neolithic pottery seems to stand in a class apart from other Greek neolithic wares. It is a fine painted ware, and the shapes are aesthetically good, as in all Greek neolithic pottery, but neither conform closely to Peloponnesian or Thessalian standards and types.

The Early and Middle Bronze Age, on the other hand, is that of Greece as a whole, and between the Neolithic and the Early Bronze Age there is the break in culture universally perceptible in Greece. Aegina is particularly rich in Middle Bronze Age finds, one vase-fragment with a representation of a human figure being a remarkable addition to our ceramic knowledge of that period. Minoan influences are perceptible later, and later still fine examples of pure ' Palace Style ' Mycenaean ware are found. The Mycenaean pottery continues without a break to the close of the Bronze Age. In the 13th century a hill-settlement was established on Mount Oros, the highest point in the island which Dr Welter attributes to invading Dryopes. Soon after it was established, at a date suggested as 1230 B.C., it was destroyed, together with the main town in the island, and Aegina appears to have been almost completely denuded of inhabitants.

Dr Welter deals with the recorded history of the island in detail and gives a most convenient account of the Aphaia sculptures. He concludes with a useful chronology of the history of the island and a bibliography, a list of cults and artists, and a useful chart of ceramic imports. Altogether an ideal handbook both for visitors and for archaeologists. S. CASSON.

THE ART OF EARLY WRITING, with special reference to the Cuneiform system. *By* CUMBERLAND CLARK. *pp.* 151, *and* 16 *plates. The Mitre Press*, 1938. 6*s*.

The author of this little book is an enthusiastic collector of cuneiform tablets who desires to communicate his enthusiasm ' to others who up to now have no knowledge of a most engrossing and inspiring branch of study '.

Early in the book (p. 15) we find the remarkable statement that the Sumerians, when they settled in Mesopotamia, brought with them ' a picture-writing which they had impressed upon flat stones and papyrus leaves '. It would be interesting to know in what country and at what period of civilization the Sumerians had learnt the use of the papyrus-plant as a source of writing material. In chapter v the author discusses the origin of the Sumerians, and with the enthusiasm which inspired him to collect cuneiform records he adopts Waddell's theories of the Aryan origin of the Sumerians and their language. He also

ascribes the origin of Egyptian civilization to ' Sumerian pioneer efforts ', (p. 57). A little farther on we read that ' in this country originated the story of Noah and of the Babylonian hero, Gilgamesh, both of whom survived the flooding of the world ; and it is not difficult to understand how or why '. We learn on p. 131 the reason for this rather perplexing statement, since we are told there that in the Mesopotamian ' Deluge Tablet ' is to be found the Babylonian account of the Flood, ' in which their hero Gilgamesh has very much the same experiences as Noah '. Presumably the author has confused Gilgamesh with his ancestor Ut-napishtim, the Babylonian Noah, the only man who, according to Babylonian tradition, survived the Deluge.

The man in the street, for whom this book is intended, is not likely to be in a position to know that Waddell's theories are extremely precarious, or that there is no evidence whatever that the Sumerians used papyrus for writing material, or that Gilgamesh never passed through the Deluge, so far as our present state of knowledge goes.

It is to be regretted that the author's enthusiasm has not been a little better informed with elementary knowledge. His intentions are admirable, and there is much useful information in his book, but what is reliable is so intermingled with misleading statements that it is impossible to recommend the book to the class of reader for whom it is intended. S. H. Hooke.

A REPORT ON THE MUSEUMS AND ART GALLERIES OF THE BRITISH ISLES (other than the National Museums). *By* S. F. Markham to the Carnegie United Kingdom Trustees. *Edinburgh : T. and A. Constable*, 1938. *pp.* 179 *and* 24 *plates.*

The Carnegie Trustees have again earned our gratitude by the publication of this comprehensive Report, which enables us to judge of the progress made by Museums as a whole during the ten years which have passed since the appearance of the Report by Sir Henry Miers.

Undoubted progress has been made ; there are more good museums and more museums striving to be good. It is also apparent that the public, when given a chance, appreciates good service in museums as well as in other spheres.

It is cheering to read that more museum authorities have realized their threefold duties of conservation, research and education. Perhaps the most noticeable improvement is in showmanship. Apart from such costly features as the diorama, there is a widespread and successful effort to display ordinary museum objects to better advantage. Curators are realizing what the proprietors of fashionable hat-shops learned years ago, that the spectator is merely dazed by the overpowering effect of too-crowded show cases, that attention can be called to a particular object by placing it in reasonable isolation and that a good

light is necessary for objects to be seen. It is also being realized that the museum visitor has the right to be able to learn what he is looking at (without the tiresome necessity of buying a catalogue) by means of legible and concise descriptive cards and labels.

A striking example of old and new methods at the Liverpool Public Museums is given on plate 17. The old, with its heavy shelves, cluttered with far too many objects, and the new with its pleasing and dignified arrangement on tiers of rectangular blocks. (But even with this admirable arrangement, there is not a single label—nothing to tell the visitor what the objects are, what they mean or where they come from).

Progress there has certainly been, but there is another side to the picture. The majority of museums are suffering from most serious handicaps—inadequate finances, unsuitable buildings, insufficient staff, or all of these. How can such institutions, however hardworking and keen their staffs, really carry out the purposes of a museum ? Again there is a residuum of really bad museums, those which have given up the struggle, with curators too indolent to attempt any improvement, controlled by committees ignorant of the very purposes for which museums exist.

Perhaps the most interesting, as well as the most controversial, section of the report is that which gives Mr Markham's proposals for the improvement of the unfortunate, and the elimination of the bad, museums. It is no less than the nationalization of all museums. This proposal is certainly revolutionary ; it will arouse much opposition ; it can hardly be considered practicable at the present time—but it leaves your reviewer with a feeling of hope. Speaking only for himself, he would endure any number of the inevitable Government Inspectors, he would fill in any number of the inevitable departmental forms, if only he had the certainty that his museum could ' fulfil its destiny ' uncramped and unhindered by the heartbreaking restrictions that present conditions impose on all but the most fortunate of museums.

The report can be obtained free of charge by anyone interested who will apply to Comely Park House, Dunfermline. CHARLES D. DREW.